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**A Different Kettle of Fish**  
**Turning Around How Computer Modelling Counts for**  
**(Fisheries) Policy-Making**

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Doctor of Philosophy

Science and Technology Studies

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*For Tom (in memoriam)*



*... In that Empire, the Art of Cartography attained such Perfection that the map of a single Province occupied the entirety of a City, and the map of the Empire, the entirety of a Province. In time, those Unconscionable Maps no longer satisfied, and the Cartographers Guilds struck a Map of the Empire whose size was that of the Empire, and which coincided point for point with it. The following Generations, who were not so fond of the Study of Cartography as their Forebears had been, saw that that vast Map was Useless, and not without some Pitilessness was it, that they delivered it up to the Inclemencies of Sun and Winters. In the Deserts of the West, still today, there are Tattered Ruins of that Map, inhabited by Animals and Beggars; in all the Land there is no other Relic of the Disciplines of Geography*  
– Jorge Luis Borges (1998[1946]). On Exactitude in Science. In *Collected Fictions*, translated by Hurley, A. London: Penguin Books. p. 325.



## **Declaration**

I, Diego de la Hoz del Hoyo, declare that this thesis is work of my own and that the work has not been submitted for any other degree or professional qualification except as specified.

Diego de la Hoz del Hoyo

30 October 2013





## **Abstract**

This thesis examines how computer modelling matters for policy-making by looking at two case studies of European fisheries management. Based on documentary analysis and ethnographic interviews and observations, the main case is located within the European Union (EU) and centred around the flatfish fishery in the North Sea with a supplementary one from outside the EU and focused on the North East Arctic cod fishery in the Barents Sea.

As in other much-contested areas of public policy, fisheries officials in the EU and neighbouring countries seek to develop a universalistic and objective ground by which to depoliticise management decisions. In this sense, modelling has long become their preferred approach to produce policy relevant representations of the otherwise hidden dynamics of a fishery.

Social constructivists in the field of Science and Technology Studies (STS) studying the modelling used in areas of policy-making such as, for instance, climate change have questioned whether models are the right tools for this job given that the modelling may conceal large uncertainties about their accuracy and relevance to policy-making. Some of these scholars argue for producing ‘good’ models for policy-making, and thus more robust policies, by constructively engaging the non-modellers or non-specialists in the quality assurance of the modelling. ‘Fisheries Studies’ literature suggests, however, that modelling can contribute to policy resilience despite its well-known limitations to produce accurate fish counting. It follows that models are doing something else than providing policy-salient real-looking representations.

How may modelling count differently for policy-making in fisheries and beyond? Drawing on the ‘co-production’ of science and social order framework from STS, the thesis puts forward three related arguments. First, that the technologies designed to depoliticise decision-making, including modelling, become spaces for political work

by policy-makers, stakeholders and scientists. Second, that the role of computer modelling for policy stems from how representational validity and political usefulness are produced together. Third, that the role of modelling for policy is mediated by virtue of being assessed together with other technologies for depoliticising as part of a whole sociotechnical infrastructure to allow evidence-based decisions. As a distinctive contribution, this thesis thus questions the presumption in many social constructivist accounts that modelling alone becomes central to the policy process and its outcomes. The significance of modelling for policy-making should be understood in terms of its contribution to processes of sociotechnical framing. Narratives that foreground the former and background the latter show an analytical bias that needs turning around.

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## List of Abbreviations

ACFA	Advisory Committee on Fisheries and Aquaculture
ACFM	Advisory Committee for Fisheries Management
AFWG	Arctic Fisheries Working Group
ANT	Actor-Network Theory
CBM	Community Based Management
CEC	Commission of the European Communities
CFP	Common Fisheries Policy
CLRTAP	Convention for Long Range Transboundary Air Pollution
CLWP	Commission Legislative and Work Programme
COMMIT	Committing to Tailor-made Long-term Fishery Management Strategies
DEFRA	Department for Environment, Food and Rural Affairs (United Kingdom)
DG-FISH	Directorate-General for Fisheries and Maritime Affairs (European Commission)
DG-MARE	Directorate-General for Maritime Affairs and Fisheries (European Commission)
DIFRES	Danish Institute for Fisheries Research
EEC	European Economic Community
EEZ	Exclusive Economic Zone
EFIMAS	Operational Evaluation Tools for Fisheries Management Options
EIAA	Economic Interpretation of the ACFM Advice
EU	European Union
FAO	The United Nations Food and Agriculture Organisation

FCCC	Framework Convention on Climate Change
FOI	Institute for Food and Resource Economics (Denmark)
GCM(s)	General Circulation Model(s)
HCR(s)	Harvest Control Rule(s)
IAM(s)	Integrated Assessment Modelling(Models)
ICCAT	International Commission for the Conservation of Atlantic Tunas
ICES	International Council for the Exploration of the Sea
ICNAF	International Commission for the Northwest Atlantic Fisheries
IIA(s)	Integrated Impact Assessment(s)
IMARES	Institute for Marine Resources and Ecosystem Studies (The Netherlands)
IMR	Institute of Marine Research (Norway)
IPCC	Intergovernmental Panel on Climate Change
ISH	Institute for Sea Fisheries (Germany)
ITQ(s)	Individual Transferrable Quota(s)
IUU	Illegal, Unreported and Unregulated
JNRFC	Joint Norwegian-Russian Fisheries Commission
JRC	Joint Research Centre
LEI	Agricultural Economics Research Institute (the Netherlands)
LNv	Ministry of Agriculture, Nature and Food Quality (the Netherlands)
MPA(s)	Marine Protected Area(s)
MSE(s)	Management Strategy Evaluation(s)
MSY	Maximum Sustainable Yield
NEA	North East Arctic
NEAFC	Northeast Atlantic Fisheries Commission

NFA	Norwegian Fishermen's Association
NGO(s)	Non Governmental Organisation(s)
NSCFP	North Sea Commission Fisheries Partnership
NSRAC	North Sea Regional Advisory Council
NWWRAC	North Western Waters Regional Advisory Council
PINRO	Polar Research Institute of Marine Fisheries and Oceanography (Russia)
PROST	Projections Stochastic
RAC(s)	Regional Advisory Council(s)
RAINS	Regional Air Pollution Information and Simulation
RIVO	Netherlands Institute for Fisheries Research
SSB	Spawning Stock Biomass
STECF	Scientific, Technical and Economic Committee for Fisheries
STS	Science and Technology Studies
TAC(s)	Total Allowable Catch(es)
UN	United Nations
UNCLOS	United Nations Conference on the Law of the Sea
UNCED	United Nations Conference on Environment and Development
VMS	Vessel Monitoring System
VNIRO	Russian Federal Research Institute of Fisheries and Oceanography
VPA	Virtual Population Analysis
WSSD	The United Nations World Summit on Sustainable Development
WWF	World Wildlife Fund



# 1 Introduction

*There are these two young fish swimming along, and they happen to meet an older fish swimming the other way, who nods at them and says, "Morning, boys, how's the water?" And the two young fish swim on for a bit, and then eventually one of them looks over at the other and goes, "What the hell is water?"*  
(Wallace, 2009:3)

To state it as succinctly as possible, what follows is an ethnographic two-case study about the role of computer modelling in European fisheries policy-making using the conceptual tools and frameworks of the field of 'Science and Technology Studies' (STS). Every bit of information encapsulated in this short outline of the research journey ahead of us demands an introduction and this is what I shall do in this chapter. Why two ethnographic case studies? Why computer modelling for policy-making? Why EU fisheries? Why Science and Technology Studies? I will account for the choices being made in reverse order and then follow on with details related to the general structure of the thesis.

Taking these questions in reverse order allows me to start with a personal note on the general reasons behind doing this thesis. If I were a social scientist by training with an interest in science and technology I would probably need to say only a few words here. Embarking on a doctoral programme would be simply a commonsensical career step of someone who wanted to progress into academia. Yet I have approached these doctoral studies from very different quarters and also with different aims. I have a technical background in engineering, in electrical and electronic engineering to be more precise, and I developed the first part of my professional career with wires and radio waves before turning my head to the social sciences. The move was never intended as a genuine career change. The initial motives were more to do with developing an interdisciplinary professional profile and what I can now express as the intellectual challenge of learning to 'alternate' (Collins, 2004) across the so-called natural science-social science divide – or, to put it in less Herculean terms, between different worlds.



The field of STS offers the possibility to train in social sciences following an established tradition that has already seen natural scientists and engineers taking this path in their quest for a better understanding of sociotechnical change. By and large, it provides a fertile ground to develop cross-disciplinary profiles. Yet enrolling in an STS PhD programme somehow exceeds the mere idea of alternation. It demands taking up the venture of producing ‘contributory expertise’ in the field of Science and Technology Studies. Like we learn from the sociologists of expertise Harry Collins and Robert Evans (2002), this is quite a different ball game from just being able to borrow and discuss some of the main lessons of a field<sup>1</sup>. I shall call this challenge number one.

The next question is about the topic of research chosen and the timing for such a topic. Why European fisheries policy-making? I must admit that when I started to take the first steps leading to the planning of this thesis work around the year of 2004 I had only a culinary interest in fish. Obvious matters in fisheries were not yet that obvious to me – as far as the introductory quote goes, I did not know what was ‘water’. This was a conscious choice though. As many practitioners in the field of Science and Technology Studies I fancied something entirely new and picked this research topic to put to test my qualitative analytical skills for cracking complex foreign problems from scratch. Starting from ‘no expertise’ was indeed paramount to seeing how much ‘interactional expertise’ I could manage to achieve in the course of my qualitative research. I shall call this challenge number two.

My fixation with fish started, as many things in life, in a serendipitous manner. Back in 2004 when I was thinking of possible research themes a book called ‘The End of

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<sup>1</sup> Collins and Evans distinguish between two different types of expertise: ‘contributory expertise’ and ‘interactional expertise’ (Collins and Evans, 2002). The former is, so to speak, what one needs in order to be offered a job in a particular field. The latter allows following a discussion in that field, to alternate between worlds as Collins (2004) puts it, but it is not enough to make regular substantive contributions.

the Line: How Overfishing Is Changing the World and What We Eat' by the British environmental journalist Charles Clover (2004) landed in my hands. That was only a few weeks after I had been in Tsukiji market, a popular destination for tourists in Tokyo. Clover's book not only offered me valuable insights to grasp what it was all about with the Bluefin tunas that I had witnessed in Tsukiji in the very early hours of the day, but also opened my eyes to the puzzling world of fisheries management, where balancing out conservation and exploitation of the fish stocks seemed generally a challenging mission – conservation was simply losing out to the long chain of business profiting from fish in many parts of the world. Clover's book may not be the kind of book shelved at the university library but, in 'detectivesque' style, it provides clinical details of the large range of actors involved in fisheries management, including the fishing industry – the villains in the story – scientists and, coming to a 'corner' of the world of personal interest, the European Union (EU).

Engineers and STS practitioners alike are thrilled by cracking complex problems and, taking a look at other sources, fisheries seemed enough of a hard case at first impression; a sheer example of 'managing the unmanageable', as it was concisely put by commentators. This was already an inspiring reason to consider it as a possible area for a research topic. In addition, when it comes to the EU, I found it stimulating to realise that the fact that fisheries is a special area where Member States share a common policy – with the authority over the fish stocks centralised in Brussels – added to the complexity. I also discovered during this exploratory phase back in 2004 that many of the fish stocks swimming in EU waters were overexploited, or so the daily newspapers also reported – Charles Clover was not alone in this regard. And reading a little further, I learned that the timing was probably ideal for the study of EU fisheries since the policy-makers in Brussels had recently approved a major reform in 2002 precisely to deal with the crisis picture of diminishing fish stocks. By and large, it did not take too long before I realised that my visit to the Tsukiji market in September 2004 and reading Clover's captivating book shortly afterwards had already made me fall for an exciting area of inquiry with European significance where I could confront the two challenges mentioned above.

Moving to the next question, it is the turn of computer modelling. The first thing to say is that computer modelling seemed a prevalent element of the governing machinery in European fisheries management. As such, it made a suitable entry point for a STS study, which usually throws the spotlight on a particular piece of science or technology. In addition, there was already a consolidated amount of work in the social constructivist strand of STS around computer modelling and, therefore, an opportunity to speak directly to a body of existing findings. What is more, scientific modelling offered a liaison with my technical background. In STS being able to penetrate the technical discussions held by actors on the ground is highly regarded as an advantage, almost the hallmark of a field that many times prides itself on being scientific about the practice of science<sup>2</sup>. Having some sort of technical background often helps the STS analyst to boldly decipher a broad range of scientific debates. In my case, scientific modelling was the bit of the opening statement to this chapter that I felt conveniently close to my trade as an engineer.

However, it is important to bear in mind that modelling in engineering can have a different significance to that in science. In her book ‘The New Engineer’, Beder (1998) provides a fairly recognisable picture for those in the profession when it comes to modelling. Faced with complex problems, she points out that engineers will quickly resort to mathematical models and computer simulations to find a fitting enough solution. In this sense, models are not expected to represent the real world – only a handful of key features that are considered relevant to the problem are included. The aim of the model is precisely not to try to reproduce reality in all its complexity but to capture what is essential to understanding some key aspect of a problem. Therefore, models are inevitably simplifications, a heuristic – i.e. something that provides a way forward in the solution to a problem but it is ultimately loosely defined and fallible. As follows, an engineer’s take on modelling is fundamentally pragmatic, preferring a rule of thumb than something that would be

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<sup>2</sup> This is for instance the dictum of the so-called ‘Edinburgh School’ within Science Studies (see Barnes et al., 1996).

too effortful, time consuming and expensive if worked out more scientifically. All in all, engineers do not require that their theories are true to use them, it is okay that they provide an ‘adequate enough’ heuristic sustained by practice. For engineers exactitude is not particularly relevant since they may have to work with other approximations anyway. Following Beder, this is in principle a different attitude to that of the scientists, who tend to favour an accurate theory over a simpler one. In other words, engineers and scientists, being part of different knowledge cultures, may show different judgements to what is ‘accurate’, ‘useful enough’, ‘simple enough’ or ‘too complex’.

In this light I approached the analysis of the role of computer modelling for fisheries policy decisions at the outset of this thesis work without taking for granted that the modelling style would be entirely familiar. Indeed, a first glance at the scientific literature revealed that, when it came to European fisheries policy, being able to tell accurately how many fish swim in the sea seemed paramount. Representation was apparently the central role of the modelling. Counting fish was mostly the realm of marine biologists who, with the rise of computing capacity and advances in computing science, could develop at the turn of the 21<sup>st</sup> century much more sophisticated computer models to match the perceived complexity of the fisheries’ dynamics. Simplicity in fisheries modelling seemed only contingent on the computing means of the past, which did not allow the representation of complexity. Against this backdrop, the research issues that seemed to be more salient were those related to validation. How did the more sophisticated tools get validated and taken up? How to balance simplicity and complexity<sup>3</sup> for representational purposes? How much simplicity or complexity was enough, not only for the modellers but also for

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<sup>3</sup> Some would have used here the term ‘complicatedness’ instead. While acknowledging that complexity and complicatedness mean different things in certain academic circles, they are used indistinctively in common language and this is what I will do throughout the thesis. Indeed, complexity theorists disaggregate the term complexity to refer to either emergent patterns of complex orderly behaviour out of very simple rules – ‘simplistic complexity’ – or complex social structure – ‘complex complexity’ (Byrne, 2005), which can only make things more ‘complicated’ to the readership of this thesis.

stakeholders and policy-makers outside the modelling circles? This line of inquiry borrowed from the social constructivist tradition within STS that had explored already – though not so much in fisheries – the limits to validation when it came to the representations produced with computer models and simulations.

Yet during the first rounds of analysis I started to rethink the research questions and recast them with a more pragmatic flavour following what I had observed and learned in the field. Was it really accuracy in computer modelling that mattered for fisheries policy-making or was it more a question of being somehow useful to the decision-making process; did usefulness depend on specific design of the modelling tools or was it more a contextual outcome of political activity; and was the role of computer modelling pivotal to the policy process and its outcomes or was this presumption a reductionist take on the policy process? These were questions that look beyond the inner construction of computer models and had somehow received less attention in the social studies of modelling literature within STS. In addition, I thought these were sensible questions that, after all, an engineer by training doing STS work on the role of modelling for fisheries policy could find appealing without feeling too much ‘like a fish out of water’.

When it comes to the methodological choice of ethnographic case studies, the last question that stems from the opening outline, this is often the option of choice to analyse any contemporary real-life problem in its natural environment. In particular within STS, it is the preferred method to demonstrate the contextualisation of science and technology and problematise claims of universality of the scientific method. It is indeed attention to context, which in quantitative studies is much more difficult to calibrate, what provides case studies their edge. At the same time, the case study method is not without its own shortcomings; especially because it is not always fully-fledged what a case is a case of – in other words, how to move from the particular to the general.

Meanwhile, I should also say at this early point that my fieldwork did not stem from the clinical implementation of a firmly structured research framework informed by theory. It was much more of a learning process ‘as one goes along’. At the end of the day, method in the Greek etymological sense refers to ‘going after’, the connotation being that of a journey. And every journey has its turnarounds. This does not mean a lack of methodological rigour nonetheless. There are pragmatic reasons behind this way of proceeding, the most important one being to be able to walk into the field ready to find out as much as possible from the actors without imposing any theory-driven frames and categories on them.

Now that the research choices have been exposed, it is time to anticipate where this research journey will lead in particular. Social constructivists in the field of STS studying the modelling used in areas of policy-making such as, for instance, climate change have questioned whether models are the right tools for policy-making given that they may conceal large uncertainties about their accuracy and policy relevance. Some of these scholars argue for producing ‘good’ models for policy-making, and thus more robust policies, by constructively engaging the non-modellers or non-specialists in the quality assurance of the modelling. ‘Fisheries Studies’ literature – sometimes in conversation with STS scholarship on the social studies of finance – suggests, however, that modelling can contribute to policy resilience despite its well-known limitations to produce accurate fish counting. It follows that models are doing something else than providing policy-salient real-looking representations.

In this sense, the thesis puts forward three related arguments. First, that the technologies designed to depoliticise decision-making, including modelling, become spaces for political action by policy-makers, stakeholders and scientists. Second, that the role of computer modelling for policy stems from how representational validity and political usefulness are produced together. Third, computer models alone – that is, the artefacts – rarely speak directly to policy. The role of modelling for policy-making is mediated by virtue of being assessed together with other technologies for

depoliticising as part of a whole sociotechnical framing to allow evidence-based decisions. Therefore, the role of modelling for policy should be understood in terms of its contribution to this sociotechnical infrastructure.

As follows, in this thesis I will foreground the sociotechnical ensemble within policy process as a unit of analysis. In adopting such a ‘wide-angle lens’, it should not be surprising I arrive at several tangential insights that are only loosely coupled with the study of the role of modelling for policy-making. This is what I call the ‘by-catch’ of my research – just like the fishermen when they go after one particular type of catch and find as well other species in their nets. As I shall show, the ‘weightiest’ by-catch has to do with the reflection on how EU fisheries are made governable through policy instrumentation.

Now that the research choices are clear and the main and tangential outcomes are upfront, it is time to detail what will be the structure of the reminder of this thesis. In Chapter 2 I will provide the ‘nautical chart’ detailing the scholarly literature(s) that have guided my research journey. A central element of the chapter will be the introduction of the main analytical framework informing this thesis, that of the co-production of science and social order, which deals with the emergence of new sociotechnical formations in tension with existing structures. The second important aspect of this chapter is that, at the end of it, I will offer some further refinement of the main research issues of this thesis.

In Chapter 3 I will provide some methodological considerations. Right after the setting of the analytical framework and main research issues and right before the empirical chapters, it will illuminate in a reflexive style the selection of two case studies and the process of collecting and analysing the qualitative data. It should read very much as a stand-alone *ex-post* justification piece of the different choices that I

have made along my research journey in order to ‘capture’ the sociotechnical framing within the policy process – the declared unit of inquiry and analysis.

In Chapter 4 I will set the stage for the first and primary case study by providing an in-depth account of problematics of EU fisheries policy up to the turn of the 21<sup>st</sup> century, and the solutions that were proposed with its reform in the year of 2002. I will describe how in parallel to reform of the EU fisheries policy, the arrival of the European Commission’s Better Regulation internal strategy in 2002 and the international agreements following the UN World Summit on Sustainable Development in Johannesburg also in 2002 had a significant influence as well. Notably, although I will draw on existing literature to contextualise the arrival and development of EU fisheries policy, I will also refer to substantial first-hand empirical evidence to inform the changes in 2002, to the point that I consider this to be the first empirical chapter.

Following with the empirical material, in Chapters 5, 6 and 7, I will introduce and develop the two case studies. The main case study will address the introduction of a troika of policy instruments as part of the new sociotechnical framing set in motion after the reform in 2002 to depoliticise the management of the North Sea mixed flatfish fishery<sup>4</sup> in the North Sea. The instruments at stake were – in the order of appearance in this thesis – integrated assessment modelling, integrated impact assessments and new fishing regulatory rules. This case study will span over the three chapters, with each of them dedicated to an instrument of the troika in order to describe how it worked. The second case, more of subsidiary one, will be self-contained in Chapter 7 and will offer insights from the Norwegian-Russian shared North-East Arctic cod fishery in the Barents Sea, particularly on how some fisheries management rules similar to those of new currency in the EU worked. Notably,

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<sup>4</sup> The flatfish fishery in the North Sea encompasses plaice and sole fish and, in this respect, it is said to be a ‘mixed’ fishery. I shall offer more details below.



modelling was pervasive in the building of the sociotechnical framing in the two case studies, and at points turned into the very object of the policy process that tried precisely to set up the new framing. In those particular instances I will discuss the modelling thoroughly in order to capture the political work that took place on the back of it. However, it is important to bear in mind that the unit of analysis will be the sociotechnical framing throughout this thesis.

In Chapter 8 I will end with the conclusions for the thesis, making the case for how each of the instruments of the troika of instruments worked, as well as providing insights into how the troika worked as a whole, as part of the sociotechnical framing to enable EU fisheries policy decisions. Following from this analysis, I will also address the more general question of how EU fisheries are made governable, what I refer to as the weighty by-catch of my research journey. Finally, I will question the presumption in many social constructivist accounts that modelling alone becomes central to the policy process and its outcomes. The significance of modelling for policy-making should be understood in terms of its contribution to processes of sociotechnical framing. Therefore, I will argue, the analysis of the role of modelling for policy needs to be decentred.

## 2 Literature review

### 2.1 *Introduction: Departing coordinates*

Put in metaphorical terms, in this chapter I will offer a ‘nautical chart’ for the research journey by mapping the scholarly literature(s) that shall guide the analysis. I will begin with the more general avenues and end with narrower conduits that inform more closely the empirical material that will be presented afterwards in the thesis. Yet, beforehand, in this introductory section I shall commence with another personal note that informs the content of this chapter and the following one on methodology.

I have already explained that I crossed to the field of STS from engineering and, understandably, it was the accounts of engineers by STS scholars that interested me in the first place. For instance, Callon (1987) observes that, voluntarily or not, engineers often become ‘engineer sociologists’. After all, engineers address social and technical problems simultaneously<sup>5</sup> (Law, 1987; see also Beder, 1998). Perhaps not surprisingly, I became quickly fond of Actor-Network Theory (ANT), the signature contribution of Michel Callon, Bruno Latour and John Law to the field (Callon, 1986; Callon and Latour, 1992; Callon and Law, 1995).

Notably, when I took my first steps in STS these scholars had already reframed to a certain degree the ethos of their approach in ‘After-ANT’ (Law and Hassard, 1999), following the sharp criticism of their original sociological programme by sociologists of scientific knowledge<sup>6</sup> (Bloor, 1999; Collins and Yearley, 1992; Schaefer, 1991)

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<sup>5</sup> Along the same line, Law talks of ‘heterogeneous engineers’ in the following terms:

[A] technical or an architectural decision is also social in character. It is both of these things. It is heterogeneous. What we need, then, is not just engineers. For the same reason we don’t just need sociologists either. What we need instead are heterogeneous engineers. Indeed, I suggest that this is a new profession that needs to be invented (Law, 2011:7).

<sup>6</sup> Cutting it short, for the sociologists of scientific knowledge ANT scholars’ inclusion of non-verbalised reality in their accounts of the process of closure of scientific controversies equalled going

and other scholars in the field of STS<sup>7</sup> (Haraway, 1991; Jasanoff, 2004; Jorgensen and Sorensen, 1999; Star and Griesemer, 1989). Rather than a sociological theory *per se*, the forefathers later suggested that ANT is essentially a toolkit of concepts to follow scientists and engineers in their ordering of the world in ethnomethodological style<sup>8</sup> (Latour, 1999, 2005; see also Yearley, 2005).

While the pragmatism behind ANT has partly inspired my research journey, the fact is that I have not leant myself to ‘applying’ the characteristic ANT vocabulary<sup>9</sup> – with some few exceptions<sup>10</sup>. Not only the terminology of ANT can be considered excessively rich to describe the practice of scientists and engineers (Lynch, 2001, Yearley, 2005; see also Jasanoff, 2012), using ANT has become overall unexcitingly formulaic<sup>11</sup> (Law, 2012; see also Lynch, 2012). Understood just as an approach to illuminate how to go about the study of actors (Latour, 2005), it makes sense to come

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back to the very scientific realism that they had been long fighting against. By and large, ANT as spelt originally cannot be considered a sociological theory by the social constructivist strand in Science Studies (see Yearley, 2005).

<sup>7</sup> An early line of critique of the foundational ANT works was the ‘managerial bias’, making reference to the fact that the narratives orbited around the entrepreneur, the champion, the scientist, the engineer (Star and Griesemer, 1989; see also Haraway, 1991). Years later Latour pointed out that the approach should serve all actors the same way: “[ANT] is a very crude method to learn from the actors without imposing on them a definition a priori of their world-building capacities” (1999:20). Meanwhile, Jorgensen and Sorensen (1999) argued that ANT does not pay enough heed to competing networks – cf. the later Callon (2006). More recently, Jasanoff (2004) criticises how the classic ANT accounts equal the size of the network to its power and stability, which seems an oddly realist stand.

<sup>8</sup> Ethnomethodology under this light is to be understood as follows: “[A]ctors know what they do and we have to learn from them not only what they do, but how and why they do it” (Latour, 1999:19). I will come back to this point in Chapter 3.

<sup>9</sup> Something not recommended anyway by Bruno Latour: “[ANT] might be useful, but only if it does not ‘apply’ to something” (2005:1, emphasis in the original; see also Law, 2012).

<sup>10</sup> One of the exceptions when it comes to jargon is the notion of ‘black-box’ (Latour, 1987). ANT scholars borrowed from the scientists and engineers the idea of black-boxing to refer to how the end-products of science and technology are taken up incontestably. The term has become currency within STS. Indeed, the STS motto reads that if you open these black boxes you will understand the outcome of the process they are part of in business, policy and everyday life – i.e. the conditions of felicity of those black-boxes to work (Lynch, 2001).

<sup>11</sup> Lynch’s diagnosis is telling:

[S]peaking as an editor who has read upwards of 200 new submissions per year over the past decade, I have been led to the sad conclusion that the volume of BADANT (Banal and Derivative Actor Network Theory) greatly exceeds the well-researched and broadly informative written work that rides under the ANT banner [...] ANT is by no means the only theoretical resource that has suffered this fate (2012:452).

back to ANT explicitly in the discussion of methodological considerations in Chapter 3.

In the meantime, it is time to introduce a few more details about my research aims. In my study of the role of computer modelling for decision-making I will focus on a modelling-intensive area of public policy in Europe, namely fisheries policy at the EU level. Notably, this is one of the very few areas governed directly from Brussels by virtue of the EU Treaties – which grant authority to the EU institutions and, in particular to the Council of Ministers, to make decisions over the fisheries resources across the ‘exclusive economic zone’ (EEZ) of the European Union. Modelling-based scientific advice is part of the whole policy process to come up with decisions that allocate fishing quotas to EU Member States on an annual basis. This is a massive top-down endeavour and, as an attempt to govern fisheries centrally on the European scale, it is an enterprise that demands a huge scientific infrastructure.

As I shall describe, this scientific apparatus was nonetheless problematised by different sets of actors over the years, thus leaving unfulfilled the project of delegating the governance of EU fisheries to the machinery of modern fisheries management on the expectation that the fish stocks could be managed mechanistically to achieve optimal socio-economic benefits out of the sea. I will throw the spotlight on the new solutions orchestrated to tackle this problem around 2002 in the context of policy reform in EU fisheries with a case study of the new management approach to the flatfish fishery in the North Sea – which was a singular case, as I will describe.

Moving the focus away from the modelling alone, I will show in the empirical chapters how, closely intertwined with computer modelling, there were as well other instruments that form part of the depoliticisation strategy. This was the case of integrated impact assessments and new rules for the distribution of fishing rights. I

will argue that the role of modelling for policy cannot be understood in isolation from these other elements. In this respect, I will refer to a troika of instruments for depoliticisation operating in the context of the design and implementation of a new long-term management plan for the flatfish fishery in the North Sea.

The structure of the chapter is as follows. In §2.2 I will review some of the relevant STS literature on the relationship between technology and politics. In §2.3 I will proceed likewise with the relationship between science and the policy process. Both sections will help to situate my analysis within two main avenues of scholarship with STS. In §2.4, I will inform the framework that I will use in my analysis. In §2.5 I will introduce the three relevant literatures that the case studies will touch on by virtue of the troika of instruments; first, the social studies of computer modelling, second, the policy studies of integrated impact assessments and, third, the social studies of fisheries management and its instrumentation. In §2.6 I will conclude with a refinement of the main lines of inquiry in the thesis.

## **2.2 *Technology and politics***

This thesis is, in the first place, about some technologies of governing and how they work. Therefore, the mapping of the relevant literature has to begin with the relationship between technology and politics, a common currency in Science and Technology Studies.

There are two major ways to approach the study of technology: the positivist stand and the STS-characteristic social constructivist one. Within the positivist view, technology is seen as an autonomous force in society; how technology works derives exclusively from the intrinsic properties of technical machines and processes. This positivistic view, somehow still dominant in our society, gives way to a common bias in the conceptualisation of the relationship between technology and society, that of ‘technological determinism’. The notion of technological determinism

encompasses both the assumption that technology develops autonomously, following an internal logic that is shielded from external control, and the belief that technology shapes society by having economic and social impacts but not the other way round (see for instance White, 1978). Technological determinism thus implies that technological change cannot be subject to political debate, thereby encouraging political inaction (MacKenzie, 1990). Under this mind-set, policy-makers can only hope to anticipate the arrival of particular technologies and prepare society for their impacts, claim the social constructivists (Bijker, 2006; Bijker et al., 1987; MacKenzie and Wajcman, 1999[1985]; Williams and Edge, 1996).

Social constructivists have indeed proved technological determinism empirically wrong. The ‘enemy’ to be fought for years, a vast number of historical and sociological case studies have illuminated that technology is always socially constructed (Bijker, 1995; Bijker et al., 1987; Bijker and Law, 1992; MacKenzie and Wajcman, 1999[1985]). The constructivist approach stresses that technological change does not follow its own momentum or logic. It is shaped by social structures – for instance, gender, class, race – in a process of naturalisation or black-boxing. If those black boxes are opened up later on, they show those socio-economic factors at play during their design phase (Williams and Edge, 1996). In general, the argument against technological determinism has been made repeatedly and I am not intending to go down this line – though it is interesting to observe that the debate re-emerges from time to time (see Wyatt, 2008).

Closer to the discussion I raise in this work is the issue of neutrality. Positivists repeatedly portray an image of intrinsic neutrality for technology, which makes it a candidate for solving all kinds of problems. All that matters with technology is how a social or political order freely chooses to use it. In contrast to the idea of neutrality, STS social constructivists illuminate how adopting a technology shapes the existing

social and political orders<sup>12</sup> (MacKenzie and Wajcman, 1999[1985]; Winner, 1980, 1986). Notably, Winner argues that technologies have either embedded politics or are inherently political. In the first case, technologies contribute to enable certain social options and foreclose others simply because of the politics introduced in the design phase<sup>13</sup>. In the second case, technologies can only work or are strongly compatible with particular social orders that they help to sustain as well. Winner's preferred example is that of nuclear technologies<sup>14</sup>. Another example comes from Noble (1978), who shows how technology and politics are also linked with the shop floor and the control and deskilling of the labour force in industry. By and large, in adopting a technology society buys the politics as much as the technical, indeed it is appropriate to treat technology as a socio-technical product (MacKenzie and Wajcman, 1999[1985]) because technology sustains particular power structures.

However, it is important to note that some of these seminal works in the area of Technology Studies come with an imprinted bias, and that is the presumption that the politics embedded in the design phase of an artefact are necessarily carried around and diffused in a rather mechanistic fashion when the given technology is later adopted and consumed as an obdurate product<sup>15</sup>. Stewart and Williams (2005) refer to this analytical bias as the 'design fallacy' and stress that it neglects the role of user innovation (Fleck, 1993) and projects the idea of linear diffusion (Sørensen et al., 2000). It is the processes of adoption that gives shape to the definitive effects that technologies have (see Joerges, 1999). Notably, such processes have been

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<sup>12</sup> To reject 'hard' technological determinism does not amount to say that technology has not social effects (MacKenzie and Wajcman, 1999[1985]).

<sup>13</sup> Winner's (1980) account of the bridges in Long Island – albeit historically problematic (see Joerges, 1999) – is an exemplary case of how technology can operate as a solution to political order.

<sup>14</sup> The atomic bomb is the obvious example of an inherently political technology in that it presupposes a centralised authoritarian, if not totalitarian, state that can control it. Meanwhile, nuclear power can be argued to be a political technology strongly compatible with such political order since it demands in effect extraordinary measures to control the nuclear risks (see Winner, 1986).

<sup>15</sup> Woolgar's (1991) account of how the user of a technology is 'configured' in the design phase represents a classic example.

conceptualised as ‘social learning’<sup>16</sup> (Sørensen, 1996; Williams et al., 2005) and ‘domestication’<sup>17</sup> (Lie and Sørensen, 1996).

As another stream of work in the study of the relationship between technology and politics, sometimes technologies are explicitly meant to play a role in governing. ‘Political technologies’ are particularly relevant to the issues at stake in this thesis and thus deserve some spelling out. Central to this line of inquiring into the relationship between technology and politics is the analysis of different ‘technologies of government’ that grows out of Foucault’s (2007) ideas on ‘governmentality’.

For instance, influenced by the work of Foucault but also reformulating some of his ideas, Asdal offers an interesting approach to political technologies, characterising them as follows: “Political technologies are not to be understood in a [Foucauldian] context of the microphysics of power, as techniques of domination exclusively, but as tools for public involvement, for democratization, or deliberation, as well” (Asdal, 2008:13). Thus, Asdal uses the term political technologies to refer to those technologies of politics that can contribute to governing by fostering and nesting explicitly political discussion and deliberation<sup>18</sup> (see also Davies, 2006; Jasanoff, 2003b).

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<sup>16</sup> As defined by Sørensen, “[s]ocial learning can be characterised as a combined act of discovery and analysis, of understanding and giving meaning, and of tinkering and the development of routines. In order to make an artefact work, it has to be placed, spatially, temporally, and conceptually. It has to be fitted into the existing, heterogeneous networks of machines, systems, routines, and culture” (1996:6).

<sup>17</sup> Social learning can go beyond technology implementation, as captured by the idea of domestication. In Technology Studies domestication refers to the process of ‘appropriation’ of a technology or media by its users and how they shape it in such a process (Lie and Sørensen, 1996). As Stewart and Williams (2005) put it, further innovation or sociotechnical change occurs as artefacts are used. In such a process artefacts themselves are reinvented or further elaborated (Fleck, 1993).

<sup>18</sup> Along the same lines, Jasanoff (2003b) talks of ‘technologies of humility’, that is, technologies that offer a new political space, inside or outside the formal whereabouts of government.



Interestingly, Asdal also confronts her notion of political technologies with the social theorist Ulrich Beck's (1992) ideas on the 'displacement of politics' away from traditional polities. Societal change, argues Beck, is not only debated in parliament or decided by the executive government, but also created in the laboratories and industries of microelectronics, nuclear power and genetic modification. These technological developments thus lose their political neutrality while at the same time continue to be shielded against parliamentary control; "[t]echno-economic development thus falls between politics and non-politics. It becomes a third entity, acquiring the precarious hybrid status of a sub-politics" (Beck 1992:186; see also Beck, 1997). Yet the idea of sub-politics does not manage to capture how issues related to science and technology that emerge outside the formal political arena can enter the official conduits of politics. Following Asdal, "[n]otions of a politics displaced should not lead us to leave unexplored the ways in which issues get tied, or not tied, to government and to ordinary political institutions" (2008:23). Asdal takes the example of the tools for 'invited' public participation; they do not point to a displacement of politics nor are they part of the formal political arena. Instead, these policy instruments offer a space where government and those under the influence of that government come together to explore and eventually enable the possibility of governing a particular public issue. By and large, in contexts of public controversies and demonstrations the notion of sub-politics is not entirely sound; actors within the formal representation of politics take up the politically charged representations produced in techno-scientific contexts (Jasanoff, 2004), thus offsetting the idea of a displacement of politics.

Miller and Rose (1990; Rose and Miller, 1992) also take Foucault (2007) as a point of departure. They suggest drawing attention to the technologies, practices and procedures associated with knowledge and expertise through which actors are brought in line with political rationalities, that is, with the objectives of political authorities. It follows, Miller and Rose argue, that government in modern liberal

democracies can take place ‘at a distance’, inscribed in the cascade of technologies, practices and procedures<sup>19</sup>. At the same time, they observe that political rationales and the technologies of government are often met with resistance by actors, making government work for other alternatives sooner or later: “[G]overning is not the ‘realization’ of a programmer’s dream. ‘The real’ always insists in the form of resistance to programming; and the programmer’s world is one of constant experiment, invention, failure, critique and adjustment” (Miller and Rose, 1990:14).

In a related discussion, Barry (2001, 2002; Barry et al., 1995) notes as well that the Foucauldian analysis of technologies of government portrays a very instrumental idea of politics. It does not say much about the scrutiny and disruption of the conditions for government. Following with the example of government-led public involvement, for instance, Wynne (2003, 2008; see also Felt and Fochler, 2010) has contested that the top-down approaches for consulting the public can work to legitimise policy decisions. The fact is that the technologies used in politics can bring the opposite for many reasons<sup>20</sup>. Governing, Barry (2002) argues, often involves arriving at decisions under conditions of enduring disagreement and lack of solid rational justifications.

In unpacking how the technologies of government really work, Barry claims that the creation of sound governmental conditions is based on the fragility of so-called ‘metrological regimes’ that enable decisions based on measurements and calculations. This fragility derives from the susceptibility of measurement technologies to external scrutiny. As follows, in order to facilitate decisions the latter has to be either screened off to protect the metrology or managed so that it does not

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<sup>19</sup> Miller and Rose borrow here from the early ANT works of Bruno Latour and Michel Callon.

<sup>20</sup> In the vocabulary of Barry (2002), the technologies of government are designed to be ‘anti-political’, that is, conceived to close debates and steer actors towards particular objectives. However, they often end up leading to the political instead, that is, to discussion between competing rationales. I will come back to this point below.

bring excessive politicisation<sup>21</sup>. Still, these technologies may end up triggering or ‘opening up’ new political spaces for discussion (Stirling, 2008).

Not only will the points made by Barry have strong echoes later in this chapter as well as in the overall thesis, but also it is important to stress the fact that he talks of technologies of governing and their workings as part of a whole, namely the metrological regime. This has a practical analytical consequence to do with selecting the unit of analysis that may capture the fragilities that Barry refers to. Bijker (1995) names the issue for the analyst when he advocates moving from the artefact to the ‘sociotechnical ensemble’ or socio-technical infrastructure as the object of inquiry. I shall come back to this point below.

By and large, in contexts where policy instruments are used to convey scientific advice into the policy-making process – the backdrop of this thesis – the analysis of the role of the technologies of governing links directly to the relationship between scientific advice and the policy process. I shall address this important bulk of work in the next section.

### **2.3 Science and the policy process**

The second scholarly avenue that is relevant to this ‘charting’ exercise is the relationship between science and the policy process, through which authority<sup>22</sup> and decisions are produced, maintained, contested and changed. I will approach this bulk of literature by cutting across two distinctive scholarly perspectives, that of STS and

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<sup>21</sup> For instance, in the case of running an economy, “contracts need to be awarded to bodies that can be trusted. Reports and information must be released at the appropriate time to the appropriate audiences. The appropriate persons needed to be appointed to committees” (Barry, 2002:280).

<sup>22</sup> It is important to set the terminology as clear as possible. It was Weber (1964) who essentially distinguished between ‘traditional’, ‘charismatic’ and ‘legal-rational authority’, the latter often perceived as the common currency in scientific advice. I will be referring to this one throughout this thesis.

Policy Studies. Notably, the review of the different conceptualisations of the role and position of science within the policy process will lead to the selection of the analytical framework that I will use in this thesis.

In principle, scientists enjoy larger degrees of authority than other actors in society to provide policy advice. Their apparent disinterestedness and objectivity suits rather well the policy-maker wanting to make universalistic decisions. However, this linear and universal view of the science-policy relationship does not hold water in practice. Not only the division of labour blurs in certain contexts such as risk management (Lane et al., 2011), or changes between countries, policy sectors and with time (Halffman and Hoppe, 2005), but scientists also often fail to keep their apparent impartiality and tend to fulfil their own vested interests (Yearley, 2005).

Some STS scholars have argued that indeed the full set of ideal behaviour that in principle grants basic scientists their policy advisory role is not at all there when it is most needed. Notably, Collingridge and Reeve (1986) claim that there are four different reasons behind this critical view of basic researchers as policy advisers. First, scientists do not develop autonomously knowledge that only later on may become relevant for policy bottom-up. If anything the opposite is the norm. Second, policy relevant questions do rarely fall within the confines of single disciplines, sometimes leading to tension and controversy as described in the previous section. Experts from different disciplines approach the same policy relevant questions in contrasting ways, which means there is often no univocal advice the policy-makers can turn to. Third, in policy contexts science is often unable to foster agreement. Those whose interests suffer from the outcome of scientific advice will look to undermine the science by flagging issues such as uncertainty. Further research tends to increase the degree of uncertainty and sharpen the existing controversies instead of closing debates, which may well happen between adversarial scientists. Fourth, science's impartiality when it comes to alternative courses of action with high stakes is not politically feasible and indeed in these policy contexts science tends to be

significantly more conservative and utilitarian than in basic research production. These four points constitute what Collingridge and Reeve coined as the ‘over-critical’ model of the relationship between science and policy, with the former understood in a mythical sense of science as rational and objective; as their argument goes, “the reality could not be further from this myth [...] relevance to policy, by itself, is sufficient to completely destroy the delicate mechanisms by which scientists normally ensure that their work leads to agreement” (Collingridge and Reeve, 1986:ix-x).

As follows, these scholars claim to offer not only a reality check but also a realistic view of the policy process and the use of science in policy. Collingridge and Reeve argue that policy decisions take place only when opposed interests with different objectives come to halfway house agreements, settling for less than complete victory (Collingridge and Reeve, 1986:32). As for the role of scientific research in such a process it is not “the heroic one of providing truths by which policy may be guided, but the ironic one of preventing policy being formulated around some rival technical conclusions. Research on one hypothesis ought to cancel out research on others, enabling policy to be made which is insensitive to all scientific conjectures” (Collingridge and Reeve, 1986:151). If scientific advice sometimes seems to have a significant influence in policy affairs it is often because it is aligned with pre-existing political consensus among stakeholders. This implies that, while on the one hand actors try to deconstruct scientific knowledge when it does not align with their already existing views and interests, on the other hand they will take up scientific knowledge that fits with those opinions and interests. Collingridge and Reeve refer to the latter selective attitude under the term of the ‘under-critical’ model of the relationship between science and policy.

Given these two dynamics, over-criticism and under-criticism, the normative conclusion for Collingridge and Reeve is that basic science is not useful to policy on the grounds of those features that make science in principle suitable for advice-

giving. Several authors have been critical of Collingridge and Reeve's scepticism with the role of scientific advice to policy. As Yearley (2005) notes, Collingridge and Reeve provide too much of a pessimistic account, showing somehow a 'research aversion' that moves them to advocate for policy options that do not need to involve any research.

Following Yearley, Collingridge and Reeve seem to have an intellectualised and narrow notion of the kind of science that serves policy. They focus on basic research but they do not discuss the particulars of the so-called 'mandated science', a different kind of scientific business with different valuing produced for the purposes of political or judicial decision-making (Salter, 1988; see also Jasanoff and Wynne, 1998; Miller and Edwards, 2001; Waterton and Wynne, 2004; Wilson and Degnbol, 2002). Such 'regulatory science' (Jasanoff, 1990) or 'fiducial science' (Shackley and Gough, 2002; Hunt & Shackley, 1999), to use just other similar idioms, is service-oriented, policy-driven and much of its authority derives not from a formal peer review process but directly from the credibility of its authors within a broad audience. As a quite distinctive form of knowledge production, the credibility of knowledge for policy is often assessed by stakeholders from government, industry or non-governmental organisations<sup>23</sup> (Gibbons et al., 1994; Nowotny et al., 2001). Some commentators express in addition that the nature of this other kind of business often transcends science: policy questions to scientific advisors frequently show a 'trans-scientific' character (Weinberg, 1972), that is, they are questions which can be asked using the language of science but which cannot be answered by science and, "in so far as public policy involves trans-scientific rather than scientific issues, the

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<sup>23</sup> Gibbons et al., (1994; Nowotny et al., 2001) coin the term of 'mode-2 knowledge production' for what they see as a new dominant way of conducting science in terms of interdisciplinary and a policy-problem driven context, as opposed to blue-sky academic research organised within disciplinary confines that characterises 'mode-1'. However, this is a binary conceptualisation that points to a grand transition in the history of science (see Halfman and Hoppe, 2005) is not without criticism. Rip (2002), for instance, claims that the analyst has to question all the time if the changes he or she observes add up for a new form of knowledge production (see Holm, 2003 for a good example); mode-1 and mode-2 should be seen as end-of-the-spectrum organisational arrangements for knowledge production locally and historically situated.

role of the scientist in contributing to the promulgation of such policy must be different from his role when the issues can be unambiguously answered by science” (Weinberg, 1972:209).

Mandated science is understandably also open to deconstruction and can raise distrust but there are a number of ways in which agreements can be fostered, following some programmatic ideas to make science for policy more socially robust under contexts where stakes are high. Both the ‘post-normal science’ (Funtowicz and Ravetz, 1993) and the ‘third wave’ (Collins and Evans, 2002) are normative approaches aiming to improve the legitimacy of knowledge under certain conditions. Proponents of post-normal science, for instance, claim that for those problems where stakes are high and there is a great degree of uncertainty or contested knowledge, stakeholders should be part of the assessment of scientific evidence as an ‘extended peer community’. In their view, this so-called ‘extended peer-review’ can improve the quality and the legitimacy of the knowledge available for decision-making. Meanwhile, third wave scholars – partially in reaction to post-normal accounts – express general concern about a risk of moving from the ‘problem of legitimacy’ to the ‘problem of extension’. If many voices are taken on board, they argue, the process may well expand far beyond the usually tight time frames of decision-making (Collins and Evans, 2002). Therefore, post-normal science is not the solution. To circumvent the problem of extension they propose a taxonomy of expertise and suggest that only people with ‘contributory’ knowledge – regardless of whether they are ‘certified’ scientists or lay experts – should get involved in the core group of actors producing salient knowledge for a particular policy problem. The third wave has also raised criticism (Wynne, 2003, 2008; Jasanoff, 2003a). It misses out the key point that the concerns of those alienated from decision-making processes do not generally refer to knowledge affairs<sup>24</sup>.

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<sup>24</sup> As expressed succinctly by Wynne:

By focusing on the level of knowledge making, both approaches are normatively reifying new variants of the deficit of legitimacy when it comes to who gets to frame the problem in the first place. As external peer reviewers or actors with contributory expertise, stakeholders and members of the general public are asked to be part of the constrained framings and meanings of the policy institutions and their expert advisors (Wynne, 2003, 2008, 2010; see also Felt and Fochler, 2010). The central lesson from STS when it comes to science for policy is that it is not only a question of how knowledge is produced in the decision-making process, but also of how the problem definitions lead to certain dominant meanings within the policy process<sup>25</sup>.

Moving now to the field of Policy Studies, two rather different approaches over the role of science in the establishment of international policy agreements and regimes have caught the attention within the field of International Relations (see Lidskog and Sundqvist, 2011). According to the dominant thinking, the so-called ‘institutional approach’ (Young, 2002), international decision-making consists of rational-acting nation states, bargaining and trying to achieve their own agendas. Science plays an external and auxiliary role to this kind of policy process. It feeds advice and recommendations to the process of political bargaining carried by self-interested actors – for instance, nation states – on the basis of cost-benefit calculations. However, the argument goes, science does not shape the interests and decisions because political actors know that it is the ‘minimum common denominator’, the

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[Public concerns] are not predominantly concerns about being illegitimately disqualified and excluded from expert debate and decision, on a propositional knowledge-question such as “what are the risks?” [...] They are more about the presumptive hegemonic imposition of what the salient concerns [or problem-definitions] thus salient knowledge-questions, thus salient knowledges, are recognized to be in the first place, as the [stakeholder] frame of meaning of the issue at stake” (2008:23, emphasis in the original).

<sup>25</sup> For instance, and moving a little bit ahead of myself, defining the problem of managing fisheries as a one of counting how many fish swim in the sea grants the biological meanings saliency within the policy process. A compelling example of this comes from a study of the decision-making process that led to the collapse of the Newfoundland cod fishery in the early 1990s (Finlayson, 1994). In response to the dominant biological discourse, the fishing industry, together with leading scholarship in the social studies of fisheries, have repeatedly voiced that fisheries management is in the first place about fishermen, not about fish (Holm, 1999).



more conservative approach, which drives the negotiation of international regimes (see Haas, 1997).

The institutional approach is in stark contrast with the notion of ‘epistemic communities’ (Haas, 1992). The term refers to large networks of expert advisors to policy from different countries who come together under the claim that their knowledge is relevant and applicable to policy. According to proponents of this approach, such communities can be very influential in proposing, establishing and implementing international policy regimes on the basis of policy-relevant and consensual knowledge (Haas, 1992). The key issue is that the cross-national members of an epistemic community can reach agreements and exert control over information independently of their political power. It is the existence of a common scientific understanding across countries in the first place that supports directly the emergence of international policy agreements subsequently.

The epistemic communities supporters have encountered criticism for an inadequate characterisation of the relationship between science and policy, one that fits uneasily with empirical evidence coming from some of their paradigmatic cases such as the Intergovernmental Panel on Climate Change (IPCC) and its role in illuminating the Framework Convention on Climate Change (FCCC). Following Yearley (2005), the epistemic communities approach casts an idealised view of a uniformed science able to reach consensus much more readily than the interest-driven national representatives coming from different geographies. Yet this is in the best case an empirical question. For instance, in the case of climate change, research on how much greenhouse gas – notably CO<sub>2</sub> – should each country be accountable for did not lead scientists to ‘naturally’ agree but to arguments and dissent. As Yearley suggests, in the particular case of climate change, Collingridge and Reeve seem to be empirically correct with their claim that science for policy often tips towards disagreement, if only because researchers work under the influence of high stakes and this leads them to split into different groupings that safeguard particular

interests. Further, disciplinary biases also became easily exposed, for example between atmospheric computer modellers and solar scientists, for whom the role of the dynamics of the sun in their explanatory accounts of the phenomenon was entirely different. The idea that epistemic communities create some kind of leadership and momentum that helps them to influence policy-makers can be also contested in the case of climate change, where the problem mobilised the emergence of the field and the creation of the IPCC rather than the other way round (Yearley, 2005).

All in all, the two approaches within the field of International Relations fail to capture the dynamics of the role of science in the regulatory arena. Mandated science is not something external to the policy process in any of the forms presented. Yet looking at other scholarship within Policy Studies that can add some initial light over these issues, I would like to highlight the work of some political sociologists studying institutions – as well as industrial economics – and their accounts of the policy processes where science intervenes. These scholars approach regulation as the framing of problems and the operationalisation of solutions through the creation of new institutions and the setting of new policy instruments (Lascoumes and Le Galès, 2007; Smith, 2013). The operationalisation of solutions often involves mandating science as a source of authority, due to its apparent capacity for depoliticisation (Jullien and Smith, 2008; Radaelli, 1999). The role of mandated science in policy and how it acquires authority to perform any sort of depoliticisation involves ‘political work’. Coined by French political sociologists (Briquet, 1994; Lagroye, 1994), the term refers to the practices of a wide range of actors exposing their usually different values and interests as they are confronted with authority distribution, both upstream and downstream the decision-making process (Jullien and Smith, 2008). Notably, political work is relational in the sense that it allows actors to manage tensions, gearing either towards agreement or dissent (Carter, 2013b).

When public policy problems emerge the response of the governments is often the mobilisation of science as part of the policy process through policy instruments in the hope of solving the problem (Lascoumes and Le Galès, 2007). This is an exercise of political work that reframes policy problems to favour solutions informed by expert authority and legitimisation. Yet, notably, this process of depoliticisation opens for politicisation: “Although superficially this may appear paradoxical, from an analytical point of view it is [...] vitally important to consider that strategies of legitimisation based upon depoliticisation are highly political” (Jullien and Smith, 2008:21). In other words, this work places the focus on the policy process and conceptualises a twofold dynamic for the effect of sound instruments for policy – depoliticisation and politicisation. However, when these political sociologists address the relationship between science and policy-making this is often essentially portrayed and discussed in terms of mutual interplay between two spheres that co-evolve (Carter, 2013a). Their position here is also partly informed by some seminal STS work, that of the co-production of science and social order, and this is already coming close to the direction I would like to take for the conceptualisation of the policy process and the role of science therein. Yet, they pay no heed to the mutual constitution of natural and social orders in making sound decisions possible. In other words, they do not go far enough in order to capture that doing science – ordering nature – and doing politics – ordering of the social – merge into the same process not just as interacting arrows but also as parallel ones (Jasanoff, 2004).

Notably, relatively recent work in STS offers a fully-fledged framework for this kind of unravelling of the relationship between science and policy where both the co-constitution of natural and social orders and the mutual interplay of science and politics in the production of these orders find accommodation. I shall address this approach in a new section since it will be the analytical approach of choice and needs careful spelling out.

## **2.4 Setting the analytical framework for the thesis**

After mapping some of the relevant scholarship in STS and Policy Studies around the relationships between science, technology and policy – and more generally politics – I will now spend some time detailing the twofold analytical framework that I will be using systematically across this thesis, that of the co-production of science and social order – with its interactionist and constitutive strands, as offered by Sheila Jasanoff in her volume ‘States of Knowledge’ (2004).

The work of Jasanoff is characteristically rich in calls for a seamless and integrative analysis, “extended across the full sweep of science in society” (1996:397; see also Jasanoff 1990, 2005). In ‘States of Knowledge’ she spells out a comprehensive<sup>26</sup> analytical framework that serves to capture the emergence of new socio-technical formations in tension and many times conflict with existing formations. Put bluntly, in our making of the world not all the options forward are equally possible because, as also argued in Hacking (1999), the worlds that have already been constructed ‘loop back’ on efforts to construct new ones.

By and large, the co-production framework goes beyond traditional social constructivist accounts. It is not artificially reductionist towards the natural or the social, as if one was the dependent variable and the other the independent variable in linear and monocausal explanations<sup>27</sup>. The co-production approach takes contextualisation seriously by looking at practices where social structures enable a particular production of knowledge, but also by illuminating how a specific

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<sup>26</sup> That is, in Jasanoff’s words, avoiding “the trap of reductionism without falling into mind-numbing holism” (2004:38).

<sup>27</sup> Scientific practices produce outcomes that cannot be by any means referred back to social factors or discoveries in nature alone. That is the kind of symmetry that the framework of co-production encourages. As follows, it goes against the demarcation of facts and values, that is, the domain of nature from the domain of culture (Jasanoff, 2004). Notably, these kinds of demarcations have strong currency in the social studies of fisheries, together with a strong asymmetry towards the social in the explanatory work (Holm, 1999). As I shall show, the idiom of co-production can do better and provide much more balanced accounts in this field.

epistemology contributes to (re-)order the social context (Jasanoff, 2004, 2010b). In this sense, Jasanoff centrally distinguishes two distinctive strands in the analysis of the co-production phenomenon, the interactionist co-production of science and politics and the constitutive co-production of natural and social orders.

#### **2.4.1 The interactionist co-production of science and politics**

Putting it bluntly, the central assumption in the interactionist strand of the co-production approach is that what we know about the world is intimately linked to our sense of authority and legitimacy of specific actors, processes and instruments. Under the influence of the Strong Programme of the Sociology of Scientific Knowledge that flourished in Edinburgh in the 1970s, empirical case studies have demonstrated that science and technology are social practices confronted with problems of social authority and credibility (Gibbons et al., 1994; Gieryn, 1983, 1995; Nowotny et al., 2001; Shapin and Schaeffer, 1985).

The interactionist element of the co-production framework helps to highlight the myriad of mutual accommodations between science and society. It illuminates that changes in science do not take place regardless of adjustments in society and, the other way round, new social problems demand changes in the production of knowledge before they are resolved. The argument has been historically grounded by the exemplary work of Shapin and Schaffer in 'Leviathan and the Air-Pump' (1985) over the exchange between two influential figures of early modern England, Robert Boyle and Thomas Hobbes, regarding Boyle's scientific experiments in the 1660s, which is worth examining at length – as does Jasanoff (2004). In throwing the spotlight on these two great and, apparently, diametrical poles in the intellectuality of Restoration England, Shapin and Schaffer unfold that the distinction between science and politics is not so clear cut as it is often portrayed. Although nowadays Boyle is considered one of the forefathers of modern science, and the same goes for Hobbes when it comes to political thought, in close examination they both moved at their time vastly across the boundary between natural and moral philosophy, between

science and politics, in the course of their public disputes. At stake was, overall, the credibility and authority of Boyle's experimental method to produce what he defined as 'matters of fact', that is, the production of new knowledge by means of experimental replication as opposed to being the result of the flourishing of a new theory.

Even if the experimental method led to uncertainties, Boyle's approach to knowledge production was practically concerned with credibility and agreement on matters of fact than with scientific accuracy *per se*. For this very reason, he masterminded a technology of validation for experimental knowledge, the air-pump, a machine that he used to study and demonstrate the properties of air in front of other people. Notably, the space where the air-pump was put to work was conceived as a public one yet closely controlled by the conductors of the experiment. The goal was to avoid the single eye-witness and offer structured room for discussion that could expand its credibility. Since the direct witnessing or replication of an experiment was nonetheless limited to a few fellows with access to one of the scarce air-pumps – and therefore equally constrained was the credibility of the experiment – Boyle also envisaged the idea of having virtual witnesses for the experiment. By means of a rich description of the conduction of the experiment other fellow experimentalists without access to an air-pump could imagine the process and, eventually, buy into the outcome of the experimental work.

Hobbes was critical of Boyle's approach to the production of knowledge that, unlike the universal laws of geometry, required the consensual belief of an informed public; this meant taking matters of fact somewhere very close to the matters of belief promulgated by the Church. As follows, the experiment's credibility would be linked to the authority of the experimenter asking others to believe. In the eyes of Hobbes this was an open door to dissent, especially outside the circle of fellow experimentalists who could really envision the experiments: "Boyle's technologies could only gain assent within a secure social space for experimental practice [...]"

[while] Hobbes assaulted the security of that space because it was yet one more case of divided power” (Shapin and Schaffer, 1985:283). For Hobbes, a stable social order could only flourish on a substrate of universally accepted theories.

Following Jasanoff (2004), the important message put forward by Shapin and Schaffer with their account of the disputes between Hobbes and Boyle is that solutions to the problem of knowledge involve decisions of social order<sup>28</sup>. The implications of the argument for the study of interactionist co-production are that threesome, according to these scholars. First, scientific authority does not come out of an inner set of norms (cf. Polanyi, 1962; Merton, 1973, cited in Jasanoff, 2004) but out of the political or boundary-work (Gieryn, 1983) that takes place in situated contexts where what counts as science and what doesn’t, does not need to be defined. Scientists know better because they are good at boundary-work, not because they hold a set of principles and strict methods and that makes their knowledge special. It is boundary-work that informs creation, reproduction or shifting of particular division of labours between science and politics (Halffman and Hoppe, 2005; Gieryn, 1995; Guston, 2001; Jasanoff, 1990).

Second, the instruments of science and technology become political since they require – or are at least need to be strongly compatible with – a particular set of social and political divisions of labour and hierarchies – as in Winner’s (1986) example of the adoption of nuclear energy and its associated reactors for instance. Even the introduction of an instrument such as the air-pump ended up politicising; it turned the scientific experiment into a polity by bringing in new social ordering<sup>29</sup>. This politicisation of science, which STS has prolifically unfolded (see for instance

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<sup>28</sup> By and large, Jasanoff (2004) argues following Shapin and Schaffer, Hobbes was right in claiming that what we know is not so much linked to the scientific method and the production of ‘good science’ but to the distribution of authority in society (Jasanoff, 2004).

<sup>29</sup> Along similar lines, see Yearley (1988) for a characterisation of early modern science as a social movement.

Cozzens and Woodhouse, 1995; Pielke Jr., 2006, 2007), can thus be traced more than 300 years back.

The third facet of the interactionist co-production that distils from Shapin and Schaffer's account has to do with how the scientific polity relates and shapes the formation of managerial and political regimes – the wider polity. This third perspective is best exemplified by the work of Ezrahi (1990, 2004; see also Jasanoff, 2005). Notably, he takes Boyle's construction of the experimental space where credibility was to be assessed by distant virtual witnesses as model in the modern era for the creation of authority in liberal democracy (cf. Polanyi, 1962, cited in Jasanoff, 2004). The authority of science in the modern state born with the Enlightenment rested on the demarcation of scientific knowledge from emotional, moral, political or religious considerations. Science was portrayed as context-free and, in so doing, scientists and scientific institutions could in principle keep direct and explicit political interventions at bay. Yet, at the same time, preserving the authority of science from 'external' considerations by boundary-work – so that it could be regarded as objective – turned the very same science into something politically appealing for the construction of the democratic political order<sup>30</sup>. In other words, not only the political space could be rather similar to an experimental space when it came to achieving social authority and credibility, but the modern liberal state could use science and technology to depoliticise and gain credibility and legitimacy in the face of inquiries by an informed public<sup>31</sup>. Notably, this relationship

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<sup>30</sup> According to Ezrahi (2004), the impact of the scientific culture on modern politics – and particularly democracy – is signalled by, first, the growing use of technical vocabularies in areas of political discourse where before religious, moral and legal discourses dominated; second, the popularisation of mechanistic metaphors in discussions about the state or the market; third, the emergence of public accountability of political decisions, deriving directly from the peer accountability of scientific outputs; and fourth, the idea that rational consensus is possible in the political arena inasmuch as it is for the scientific arena.

<sup>31</sup> Yet the emergence of democracy and the modern liberal state during the Enlightenment also depended on the rise of the knowledgeable citizen capable to assess the legitimacy of those new political technologies of the state. By and large, the legitimacy of those instruments cannot be taken for granted; the two processes of legitimation go together under the co-production view (Jasanoff, 2004; see also Jasanoff and Wynne, 1998).



between scientific expertise and the democratic political order, which if anything has intensified after WWII, is often referred to as the ‘scientisation of politics’ (Hoppe, 1999; Maasen and Weingart, 2006; Weingart, 1999).

Correspondingly, an avenue within the interactionist co-production literature deals with the co-evolution between science and the evidence-based policy process, making the framework not only amenable for STS scholars looking beyond purely epistemological debates and political theorists but also to a range of Policy Studies commentators (see Boswell, 2009; Carter, 2013a; Lidskog and Sundqvist, 2011; Tuinstra, 2008; Tuinstra et al., 2006).

Yet, as anticipated in the previous section, to complete the analytical framework for this thesis what is needed in addition is a conceptualisation of the evidence-based policy process not only in terms of the necessary redistribution of authority but also in terms of the constitutive co-production of new natural and social orders through the processes of sociotechnical framing that trigger that redistribution in the first place. This I will address in the following subsection.

#### **2.4.2 The emergence of new socio-technical framings and their fragility in certain contexts**

The constitutive strand of the co-production framework deals with processes of emergence and stabilisation of new sociotechnical framings that illuminate new entities, representations, calculative objects that cannot be explained by recurring to the natural or the social alone – for instance, a policy relevant scientific representation such as climate sensitivity to CO<sub>2</sub>. The distinction between the natural and the social might be a helpful ‘purification’ (Latour, 1993) in the economy of language *a posteriori*, but it does not capture how representing the world and making the world are the same process (Bowker and Star, 1999; Clarke and Fujimura, 1999; MacKenzie, 2006; Pickering, 1995; Scott, 1998). As follows, the instruments for

representing the world do not reflect the real world but, in a recursive loop, produce the objects that their business of representing is about<sup>32</sup>.

This is central for explaining policy decisions based on some form of representations or calculative objects away from the technical-rational template. The rational model offers a linear sequence of phases within which the options in the real world are compared and costs and benefits calculated. Access to the real world is taken for granted by means of a technical infrastructure and the evidence produced in such endeavour is seen as neutral, disinterested or apolitical. As follows, decision-makers are apparently persuaded by the optimal and most technically plausible option<sup>33</sup>. Yet in practice decisions are enabled by what Callon (1998, 2004) also calls the ‘framing’ (after Goffman, 1961, 1974), which relies on the sociotechnical infrastructure that contribute to emerge, discipline and naturalise the natural and social orders upon which a calculation is then possible<sup>34</sup>.

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<sup>32</sup> As another example, this time from the world of fisheries management, and advancing some of the later discussion, fish stocks are not living natural entities that one can find in the sea, they are created in our society through fishing rights, biological surveys, computer models, calculations and negotiations (Johnsen, 2013; see also Holm, 1999).

<sup>33</sup> Recognising that there are practical constraints on rational decision-making, Herbert Simon developed a model of the policy process premised on the notions of ‘bounded rationality’ and ‘satisficing’ (see for instance Simon, 1972). Decision-makers, accepting limits of different sort, choose compromise policies that satisfy rather than optimise organisational goals.

<sup>34</sup> It is noteworthy the existence of sharp critics of Callon’s framework such as Daniel Miller (2002). Talking about how the relationship between market decisions and the market mechanisms portrayed in the economists’ textbooks and used for framing, he points out that the Callon’s notion of the framing is misleading. The framing is not part of the actual market mechanisms – as Callon argues – but a normative representation of the market ‘priestly’ put forward by the economists; a moral or ideological set of (‘virtual’) conditions that influence, distort, shape a market decision and but still does not explain the decision because actual decision-making on the ground is much more messy – or entangled – and thus subjected to social contingencies than what the economists’ theories and textbooks can possibly describe. By and large, market decisions are underdetermined by market mechanisms. For Miller, the framing does not create the conditions to close a market decision in practice, it is just a template; although he admits the progress of the economists in dealing with externalities affecting their abstract templates, it is simply beyond means to account for them all to represent the real world – which is too ‘hot’ to use Callon’s terms. In what was also a scholarly heated debate, Holm (2007) provides one of the counter-critical pieces of Miller’s position – see also Callon (2005) – where he argues that Miller’s account lacks the edge to interrogate the increasing power of the economists and how their theories make a difference in the economic practice.

Notably, this line of work of Callon's is particularly illuminating when placed next to the co-production framework. While co-production deals with the emergence and stabilisation of new sociotechnical framings against other existing framings, it seems to say little about how these framings may – or may not – struggle to survive the passage of time under certain contexts. These issues have been captured by Callon with his 'framing and overflowing' approach, a framework for the conceptualisation of a dynamics where there is perennial tension between emerging and existing orders. My argument is that the work of Callon can offer conceptualisation of a dynamics of framing exposing fragility (see also Law, 2006). It goes beyond the idea of emergent sociotechnical formations that tend towards stabilisation over time to reflect instead that certain framings – and thus natural and social orders – can be also fragile and constantly in the making<sup>35</sup>.

Central to Callon's approach is the notion of externality, which he uses to discuss the general mechanisms of decision-making – in economic markets and beyond<sup>36</sup>. It is Callon's conceptualisation of how existing natural and social orders can loop back. Externalities are those positive or negative effects of the governing of our affairs; in other words, "externalities are simply the results of imperfections or failures in the framing process" (Callon, 1998:249). Actors bearing the costs or profiting from the externalities may then deliberately undermine the framing responsible for producing those externalities. This Callon calls the 'overflowing' of a framing, what makes it fragile or undermined. Thus, when it comes to policy, overflowing signals those situations where positive or negative externalities shake the stability of a framing, opening up for 'discontinuities' in the decision-making process (see Strathern, 2002) and exposing the limits to the governability until some form of 'reframing', that is,

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<sup>35</sup> Thereby, I will use Callon's work wherever I need to add further analytical depth to the ideas of co-production, in particular with mid- or longer-term dynamics in the decision-making process.

<sup>36</sup> Callon (1998) extends the notion of externality to include behaviour that is not exclusively of economic nature.

the rebuilding of the framing to take care of the externalities, enables stable decision-making again.

In the remit of policy decisions in particular, the process can be assimilated to the tension between depoliticisation and politicisation; or between the political and politics as defined by Barry (2002). To recall, the political for Barry is a space for dissent; “an action is political [...] if it opens up the possibility for disagreement” (Barry, 2002:270). On the other hand, politics is to reduce and shape the space for disagreement, engineering arrangements where consensus can be achieved or where dissent is banned. Therefore, claims Barry, politics can be regarded as the ‘anti-political’. It is about creating the conditions for government<sup>37</sup>, which may be questioned and disrupted if the actors involved do not sanction the politics positively. As put by Callon in his own terms:

Politics is the area of framing, repetition, closure, of the ‘lock in’ and pre-coding of issues [...]. ‘The political’, by contrast, is the area of the openness of new sites and new sights, of difference, of ‘lock-out’, of new objects of protest, of the productions of events, or, alternatively, of overflows and their demonstration [...] The sciences and technologies contribute greatly to repetition and closure but they may also, in certain conditions, produce openness (2004:133).

It is also important to mention that Callon stresses the significance of the context in the long-term dynamics of framing and overflowing, closeness and openness. He describes that there are two ideal types of situations in decision-making. He names them in metaphorical terms as ‘cold’ and ‘hot’, inspired by thermodynamics. In cold situations actors are identified, interests are stabilised, preferences can be expressed, divisions of labour are acknowledged and accepted, salient evidence is already known or easy to identify, and therefore decisions can be made. Meanwhile in hot

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<sup>37</sup> Here government is to be understood as “a domain of cognition, calculation, experimentation and evaluation” (Rose and Miller, 1992:175).

situations everything becomes controversial, beginning with the identification of externalities by a wide variety of actors and ending with the way effects are measured:

[In hot situations] facts and values have become entangled to such an extent that it is no longer possible to distinguish between two successive stages: first, the production and dissemination of information or knowledge, and second, the decision-making process itself. Such [situations] have been proliferating ever since the emergence of the controversy over the hole in the ozone layer in 1974. The crisis relating to mad cow disease is a classic example: here, the turmoil has reached its apogee [...] Not only are the various actors and their interests in constant fluctuation, but even when they enter the debate they are incapable of reaching agreement either on the facts or on the decisions that should be taken. Framing – predicated upon the assumption that actions and their effects are known and measured – is a chaotic process, the implementation and control of which depend directly on the evolution of the controversies involved and on the construction of an agreement regarding the reality and scope of the overflows (Callon, 1998:256).

Central to the production of representations, uncertainty is one example of something that can be tolerated, disciplined, framed in cold situations but can overflow in hot situations. In a hot situation those alienated from the production of knowledge often scrutinise the scientific outputs and the scientific community has no longer full autonomy to decide whether and how uncertainty is presented to outsiders. Handling uncertainty becomes part of the general strategy for negotiating social authority in a disputed domain (Campbell 1985). That is, for establishing or maintaining a framing. Meanwhile, in relatively cold situations scientists can get away with downplaying the extent of uncertainty present in their representations, as demonstrated empirically by a number of scholars. For example, Holm (2007) shows how in the relatively ‘cool’ waters of the Norwegian sea it was possible after vast scientific, technical and institutional investments to frame the sea into an ‘aquarium’ and the wild and intractable fishes into a ‘cyborg fishes’, which one can represent, count with more or less uncertainty, and therefore control and decide upon (see also Holm, 1999; Johnsen et al., 2009). Addressing the not so relatively afar policy problem of

distributing exchangeable greenhouse emissions rights, MacKenzie (2009) describes the large framing efforts behind the setting of carbon markets. He points at the uncertainties behind assimilating the destruction of one type of greenhouse gas in one location with emissions of a different type of greenhouse gas in a different location – both needed to be boiled down to tons of the reference gas CO<sub>2</sub> in an estimate for which the authority of the elsewhere questioned IPCC works fine. Just like in the previous case, in a cool situation scientific representations hold the framing together so that is possible to (constitutively) co-produce natural and social orders. Analogously, but moving away from issues of governing commons, Pollock and Williams (2007) talk of the ‘scaffolding’ – their metaphor for framing – needed to construct a comparison in public procurement processes for technology acquisition where there is often a great uncertainty about how each of the different solutions would deliver if selected. In the same way as with the cases of the cyborg fish and the tradable greenhouse emissions, they describe how it was not the properties of a particular technological solution that drove the procurement decision but the framing put in place to make choices commensurable, at least in a cold situation.

All in all, the overarching lesson across the three examples is that truth is simply the outcome of a framing that works under relatively ‘cool’ contexts. Because the socio-technical framing works, ‘truth’ can tolerate uncertainty. Yet, no matter how well it works, overflows will sooner or later trigger a process of readjustment of the framing, of reframing.

By and large, the combination of the twofold co-production framework with the dynamics introduced by Callon’s approach is a comprehensive analytical means that will inform the study of how decisions are made in EU fisheries policy at the turn of the 21st century. I will now move on with the mapping of other relevant literatures, but this time taking into consideration the particulars of the case studies that I will introduce in the empirical chapters of this thesis.

## **2.5 The troika of (particularly) relevant literatures**

In the empirical chapters I will focus on the instrumentation supporting the socio-technical framing of EU fisheries decisions. As I shall explain, computer modelling was an important and pervasive element of the framing but it did not work in isolation. Moving away from the analysis of a single (often heroic) instrument, I will introduce a troika of policy instruments that helped to configure a new set of spaces for decision-making in the management of the flatfish fishery in the North Sea. The troika that I will throw the spotlight on consists of the already mentioned computer modelling, integrated impact assessments and regulatory rules to keep fishing under control. I will dedicate a subsection to the scholarly literatures behind each of these instruments, as this will later inform the discussion on how these tools worked.

### **2.5.1 Doing science and making policy with computer modelling**

Following their pervasiveness in different areas of science and policy, computer models and simulations have received a lot of attention in the sociology and philosophy of science. It is worthwhile distinguishing between two avenues in the literature from the outset. One examines the contribution of computer modelling to the conduct of science while the other does the same job with a contribution to the conduct of policy.

#### Doing science with computer models

Computer models are scientific technologies. Mathematical models and, especially, their close relative computer simulations represent<sup>38</sup> a new way of doing scientific

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<sup>38</sup> The distinction between models and simulations is sometimes not very clear. According to Sismondo (1999), models and simulations are far from being a homogenous category and they span over a continuum. Hartmann (1996) points that, concretely, the simulation character emerges when the equations of the underlying dynamic model are solved. Winsberg (1999) offers a taxonomy of five different categories of models in a continuum that develops towards simulations: first, ‘mechanical models’ – i.e. applied theory with general assumptions; second, ‘dynamic models’ – i.e. definition of parameters, boundary values and initial data; third, ‘ad hoc models’ – i.e. simplifying assumptions, approximations, reduction of degrees of freedom; fourth, ‘computational models’ – i.e. coding in

work<sup>39</sup> in the era of high computing capacities (Sismondo, 1999). And in those areas of science where the sole use of empirical methods is unattainable – such as anything that has to do with global-change – computer modelling has turned into “the central practice for evaluating knowledge claims” (Edwards, 2001:53).

Computer calculations have a long history science. Some of the first ‘computers’<sup>40</sup> were used to solve complex mathematical equations in fields such as nuclear physics during WWII (Galison, 1996, 1997; MacKenzie, 1998) and atmospheric sciences in the post-war<sup>41</sup> – the first simulations using General Circulation Models (GCMs) took place in the mid 1950s (Edwards, 2001). The birth of computer science can be dated to those days, and the arrival of high performance digital computing marked the late 1950s (MacKenzie, 1998). It took, however, several decades before computational methods were taken up across a wide range of disciplines in the scientific domains – mostly until high processing power became widely affordable and accessible. Nowadays, on the other hand, some claim that science lives in the age of computer models and simulations (Hartmann, 1996; Winsberg, 2010).

With the broad use of computer modelling on the rise since the 1990s a long-lasting philosophical debate has developed in the literature on the standing and inherent properties of computer modelling, particularly right after the seminal work published by Hartmann in 1996 on the functions of simulations in science. Rheinberger (1997) argues that computer simulations can be at first ‘epistemic things’ and afterwards

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order to run computer simulations; fifth, ‘models of the phenomena’ – i.e. a computerised representation through data analysis routines and graphic interfaces.

<sup>39</sup> The character of this work spans from a technique to investigate the dynamics of a system, develop hypotheses, substitute experiments, support experiments or gain understanding of a process (Hartmann, 1996).

<sup>40</sup> During WWII ‘computers’ were initially women laboriously producing numerical work on mechanical calculators (Galison, 1997).

<sup>41</sup> There is no coincidence here. Control of the weather was envisaged as a potential weapon of war. For an account of the relationship between climate science and nuclear weapons testing see Edwards (2012; see also 2001).



‘technical objects’ – i.e. tools for action – when, after having been intensely studied, they become black-boxed. A similar but somehow more dynamic vision comes from Knorr-Cetina (1997) when she argues that computer modelling is a constant process of transformation, with the model always acquiring new properties. In other words, the black-boxing of computer simulations is only temporary; there is a never-ending process of re-evaluation of their epistemic character. Knorr-Cetina talks of ‘objects of knowledge’, a category where she places models and simulations because they are always exposed to further research, leaving the label of ‘tool’ only to objects with a fixed instrumental action (Knorr-Cetina, 1997). Meanwhile, Merz (1999) argues for the ‘multiplex’ nature of models and simulations. She points out that what scientists want to find out about is not always ultimately something about the real world that the system represents but can also be something about the properties of the algorithms driving the simulation. In a slightly different turn of the discussion, Edwards (1999) claims that models and data are symbiotic; or models are data-laden, driven by the real world. Morrison and Morgan (1999) claim, however, that is precisely their use as ‘mediators’ between theory and the world what gives models and simulations their standing. In contrast, Knuuttila (2005a, 2005b) moves the focus away from representation and claims that our understanding of modelling should not be mistakenly reduced to models representing some external reality (see also Spencer, 2012). There are several ways of learning from a model even if it does not offer a truthful representation.

The issue of validation of scientific representations produced with computer models has received much attention in the sociology of science. A priori the strong influence of the naturalist tradition in science would imply that computer modelling is not normally seen to provide the same level of proof as the experimental evidence of some natural phenomena. However, as Shapin and Schaffer (1985) and other sociologists of scientific knowledge show, social factors and trust play a central role in the production of ‘true’ knowledge about nature. Computer generated knowledge is no different in this respect. Shackley (1997) points at ‘trustworthiness’ as a central aspect in the evaluation within any modellers’ community. Unpacking in particular

the process of internal validation of the General Circulation Models used in climate change research, Shackley et al. (1998) point at how the parameterisation in GCMs is a process where the modeller uses his or her own judgement informed by theory, data, intuition, computer resources, and so on. As the argument goes, trust happens to be the common strategy for internal evaluation within the ‘GCM community’. A good example comes from the work of Sundberg (2006, see also 2009), who explores two different ‘sub-worlds’ within the scientific field of meteorology: field experimentation and simulation modelling. Sundberg’s ethnographic work reveals, however, a significant difference in scale between the data sets used by field experimentalists on the one side and simulation modellers on the other. This affects both the fitting of the models to the data and the level of detail they can offer when they aim to reveal new aspects of nature. Nonetheless, Sundberg concludes, these constraints do not invalidate in practice the modellers’ claims<sup>42</sup>.

Another relevant way in which modelling may contribute to the conduct of science does not deal with the apparent representational capacity of the models to perform as ‘truth machines’ but with the role of computer models as tools for communication and learning. In this case, computer modelling is primarily judged by its usefulness instead of its capacity of truly representing an external nature (Oreskes et al., 1994). A good example of this is the work of Mattila (2005), who argues that computer models can operate as ‘carriers of interdisciplinarity’ within scientific contexts. Galison (1996), in a similar vein, accounts for the interdisciplinary work in the development of the ‘Monte Carlo’ simulation shortly after WWII to study non-deterministic systems. As follows, mathematicians, physicists, statisticians, chemists, meteorologists, engineers and nuclear weapon builders were all engaged in practices of simulation although, at the same time, each of them maintained a particular view

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<sup>42</sup> Sundberg (2012) describes similar struggles with validation across the community of modellers in astrophysics.

on what the simulation was<sup>43</sup>. By and large, when computer modelling plays this kind of ‘bounding without blending’ role and allows collective action in otherwise fragmented scenarios, it is often labelled after the notion of ‘boundary object’<sup>44</sup> (see for instance Sundberg, 2007).

Yet, computer modelling is also suitable territory to critically assess the limits of knowledge integration from different disciplines and specialist knowledges<sup>45</sup>, as evidenced by Shackley and Wynne (1995b; see also Rayner and Malone, 1998; Yearley, 2009) for the case of climate change advice, or Gough et al. (1998; see also Ishii, 2011) for the case of the trans-boundary air pollution. There are epistemological challenges to the idea of knowledge integration for scientific advice

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<sup>43</sup> Galison (1997) conceptualises the interface between knowledge domains as ‘trading zones’. Galison argues that in the solving of a new problem common ground can emerge locally, even while the different scientific disciplines or cultures involved mostly disagree on higher levels: “the trading partners can hammer out a local coordination, despite vast global differences [...] cultures in interaction frequently establish contact languages, systems of discourse that can vary from the most function-specific jargons, through semi specific pidgins, to full-fledged creoles” (Galison 1997:783).

<sup>44</sup> The term was originally coined by Star and Griesemer (1989) in the following way:

Boundary objects are objects which are both plastic enough to adapt to local needs and constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individual-site use. They may be abstract or concrete. They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation. The creation and management of boundary objects is key in developing and maintaining coherence across intersecting social worlds (1989:393)

In this sense, boundary objects can offer sustained coordination across a stable social diversity. Notably, some scholars observe that the concept can serve retrospective analysis of contexts with diverse and stable social worlds but it does not suit equally well the analysis of processes of stabilisation of facts (see Fujimura, 1992).

<sup>45</sup> It is useful to illuminate the three different types of cross-disciplinary practice characterised in the literature (Barry et al., 2008; Pohl, 2010). There is ‘multidisciplinarity’, ‘interdisciplinarity’ and ‘transdisciplinarity’. The distinction between multidisciplinarity and interdisciplinarity is that in the former case, disciplines cooperate but remain within standard disciplinary bounds while in the latter case there is the aim to achieve an integrated framing which synthesises perspectives from several disciplines. Moving in a continuum, transdisciplinarity aims to shorten the distance between formal experts and lay experts and between research and decision-making. Following Pohl: “Transdisciplinary research is usually conceptualised similar to what Gibbons and Nowotny have identified as mode-2 knowledge production, as a process of knowledge production for which people come together in temporary networks” (2010:11).

purposes. The more integration advances, the more underestimations over the degree of uncertainty across the disciplinary boundaries are exposed; social distance leads to enchantment, a phenomenon also captured by the ‘certainty trough’ model<sup>46</sup> (MacKenzie, 1990; see also Shackley and Wynne, 1995b). There are also methodological challenges. As put forward by Barry et al. (2008), commitment to a discipline means that certain disciplinary methods and concepts are constantly tried out<sup>47</sup> while undisciplined methods and non-disciplinary objects of inquiry, methods and concepts are ruled out. Last but not least, there are contextual, institutional and organisational challenges to the idea of knowledge integration, with the uneven division of labour between the natural and the social sciences in areas such as sustainable development. This comes across for instance in the way cross-disciplinary collaboration between natural sciences and social sciences has evolved on the back of environmental modelling, particularly from the 1980s onwards:

When Meadows et al. (1972) presented the modelling results of earth’s collapsing human population in 2020 – assuming stable patterns of consumption and economic, population and pollution growth – the natural and social science’s perspectives were joined in a systems dynamic model of the earth, and expressed in rates of economic growth, environmental pollution, agricultural production and human reproduction. During the 1980s the natural sciences took the lead in framing more specific problems: acid rain, eutrophication of lakes and the Baltic sea, forest decline, the ozone hole, the greenhouse effect and

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<sup>46</sup> MacKenzie (1990) identifies what he calls the ‘certainty trough’ phenomenon when multiple expertises interact and are synthesised for decision-making. MacKenzie notes that whilst the specialists at the core centre of knowledge production are aware of the contingencies involved in testing, validating and reproducing results, this awareness disappears for scientists and engineers in neighbouring specialties or user-groups. These latter take up a simplified account of that knowledge – after some of its key uncertainties and contingencies have been deleted, often purely by the economies of communication. As Collins claims, “distance from the cutting edge of science is the source of what certainty we have” (1985, typescript, quoted in MacKenzie, 1990:370; see also Collins, 1988:726). However, MacKenzie also identifies a second leg; that further away – socially-speaking – from the centre of knowledge production one may find alienated actors that raise scepticism of a different kind to the usual scientific scepticism across peers because the former is triggered by their distance to the debate in the first place. Lahsen (2005) shows how the certainty trough is nonetheless context dependent, a situated phenomenon.

<sup>47</sup> Kuhn (1970[1962]) calls these ‘exemplars’ or ‘paradigms’, giving way to his second definition of the latter term. A paradigm-as-exemplar is a problem-solving resource within a paradigm-as-culture that everybody in that particular scientific community knows about.

biodiversity loss are the concepts through which global environmental change entered the awareness of academy and society at large. The social sciences were a marginal power in framing the problems and the societal answers to it, as developed, for instance, in the frame of the international conventions and protocols of Montreal (ozone hole), Kyoto (greenhouse), Gothenburg (acid rain and eutrophication) and Cartagena (biodiversity) (Pohl, 2010:26).

Another relevant example comes from the ‘grassland biome’ project back in the late 1960s, an attempt to model holistically the grassland ecosystem to study and reveal its ecological properties. An army of researchers was behind the enterprise, trying to translate into maths everything that was eaten and evacuated by all the organisms in the biome. Following Kwa (1993), the modelling was intended as a means to transform American ecology into ‘big science’<sup>48</sup>, only to find the resistance of those who supported smaller models within disciplinary silos. Indeed, Kwa (1994) documents the demise of comprehensive computer models and the holistic representation of reality through cross-disciplinary enterprises in the early 1970s. Simulation models developed after 1973 usually had a significantly smaller domain than their predecessors for several political and cultural reasons, summarised in the idea that the manageability of nature and society broke down into a crisis at that sharp point in time.

By and large, the introduction of computer modelling in the conduct of science is not only about representing the outer world when direct observations are unfeasible but also about the shaping of scientific practice. Let us now look into the second way of reflecting on the role of modelling by turning to the policy context.

### Making policy with computer models

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<sup>48</sup> Mirroring the influence of supercomputing within large organisational arrangements in the domain of physics in the United States – for instance, the Los Alamos National Laboratory and the Lawrence Livermore National Laboratory (Kwa, 1993).

In principle, the traditionally envisaged role of modelling for policy is that of forecasting. Computer models are taken as tools with which to make predictions. Such modelling exercises consider a number of input variables and estimate their value at a later point in time using data from the past, thus enabling the policy-makers wanting to achieve specific future ends – such as maximised profits or productivity – to see where to manipulate the present settings.

Presented as such, the compelling role and apparent strong coupling of the computer modelling with the policy process has raised a lot of interest within STS – particularly in the social constructivist strand. A number of scholars in STS and neighbouring fields have looked inside policy-oriented modelling only to find laden assumptions and micro-politics instead of real-looking representations of the outer world and possible futures (Edwards, 1999; Evans, 1999; O'Reilli et al., 2012; Petersen, 2006; Shackley et al., 1999; Shackley and Wynne, 1995b, 1996). As follows, a line of work within STS has been on how the models are validated and trusted given the fair amount of (situated) social construction that modelling involves (Petersen, 2008; Shackley, 1997, 2001; Shackley et al., 1998; Van der Sluijs, 2002b; Van de Sluijs et al., 2005). In other words, STS social constructivists have systematically questioned whether computer models are the right tools to depoliticise or rationalise decisions on the promise of the provision of truthful representations given the genuine problems with validation.

The case of the 'Foot and Mouth' outbreak in 2001 in Britain, which saw the massive slaughter of animals and huge losses to the rural economy, is an illustrative example of this line of critique. Central to this outcome was the direct alignment between the modelling designed at Imperial College and the managers preoccupied with control of the epidemic viral infection to all cloven-footed animals. The modelling was not without problems. There were three different groups developing models in order to advise policy action, as presented by Bickerstaff and Simmons:

The group at Imperial College London [...] employed a deterministic mathematical model derived from experience modelling human sexually transmitted disease, which originally assumed a homogenous animal population, with no distinction being made between species and with a constant rate of disease spread from an infected source. The original analysis generated by this model was central to the government's decision to use the contiguous-cull policy. [Veterinary] [r]esearchers at the Universities of Cambridge and Edinburgh [...] developed a stochastic model that simulated the progress of the disease from farm to farm on a daily basis. The model also allowed for greater [spatial] heterogeneity. Another group of modellers, [...] based at the government's Veterinary Laboratories Agency (VLA), also utilised a stochastic model but, whereas the underlying operation was similar to that of the model developed by the Cambridge and Edinburgh team, the number of variables modelled was much greater (2004:402).

The modelling contributed to produce different versions of the outbreak; its causes and development were elements of disagreement in the advisory arena. There were strong differences in the approach between the epidemiologists at Imperial College and the other two groups of vet modellers based on their completely different spatial analysis with the latter knowing the specificity of animal health and the location of all farms. Vets also had strong scepticism about the availability and the quality of the data used by the epidemiologists. On the other side, the epidemiologists reduced veterinary knowledge to, “craft ‘experience and intuition’ with no suggestion of formalised scientific tools and techniques, in contrast to the ‘complex and seemingly abstract’ models of the epidemiologists (which, it implies, veterinary practitioners may have some difficulty understanding or appreciating)” (Bickerstaff and Simmons 2004:406, emphasis in the original). In the end, the modelling approach of the Imperial College was the one picked by the policymakers as the ‘right tool for the job’. Not that truthful representation triumphed as such. Bickerstaff and Simmons claim that, while the Imperial College team might have also been particularly skilful in lobbying the government, by having a deterministic approach their models delivered representations more rapidly and in an unambiguous manner, and thus facilitated the uptake of the modelling. In other words, the modelling at Imperial College was useful. It served well a particular politics in the context of such outbreak (Law, 2008).

The management of the ‘Foot and Mouth’ outbreak in the UK famously made headlines as ‘carnage by computer’, an expression then used by everybody – including the farmers – to dismiss the use of an ‘non-validated’ model to decide on the culling policy.

Questions of representational validity can also be acute under the scrutiny of the general public when authorities introduce computer models for the management of public affairs. In this sense, Yearley (1999), exploring explicitly whether the main research findings of the STS ‘public understanding of science’ programme apply also to the public’s understanding of computer models in public settings, offers empirical insights into the public assessment of computer-based air-quality monitoring in the city of Sheffield (UK). In the view of the public authorities, the modelling was meant to help citizens to make decisions that could be sensitive to the degree of pollution in the air – a sort of an ‘empirical friend’ (see Yearley, 1991, 1992) to decide about staying in the house or cycling to work, for instance. Yet the reaction from the public showed distrust towards the hidden agenda of the city council, the naïve social assumptions involved in the implementation of the system and the mismatch of the outputs with some of the citizens’ local knowledge. Yearley (1999; see also Yearley et al., 2001) observes that computer modelling does not necessarily add new or better knowledge and, therefore, in order to prevent validation controversies, local knowledge should be taken on board in the building of modelling at an early stage. At the same time, Yearley concludes, it is important to realise that modelling is also evaluated in the view of a wide range of social and political issues, some of which can be only loosely related to the modelling *per se* – e.g. hidden agendas.

Notably, what started as a purely academic exercise turned into policy-salient research on how including citizens as ‘extended peer reviewers’ of the quality of model-based air-pollution monitoring systems in so-called ‘participatory modelling’ focus groups could have significant policy implications (see Yearley et al., 2003; see also Dahinden et al., 2000, 2003; Darier et al., 1999; Siebenhuner and Barth, 2005;



Van der Sluijs et al., 2005; Van der Sluijs, 2002b). Yearley et al. observed that the addition of some kind of technique to register the local spatial knowledge could improve significantly the relevance to policy. In this sense, they describe how the citizens attending those participatory modelling exercises in three British cities were able to map their local spatial knowledge of air-quality problems by means of geographic information systems, which the authorities effectively used to check and validate the outcomes of the monitoring system. Reflecting at a later point on the differences between the initial exercise in Sheffield and this latter elicitation of the citizens' local knowledge through geographic information technology, Yearley (2006) points out that the engagement of the members of the public as extended peer reviewers by means of mapping did not deliver the same critical discussions over the modelling. By and large, in the first case, the citizens not only scrutinised the modelling but also went beyond discussions over the inner elements of the modelling on the basis of their substantial knowledge of the issues being modelled (see also Landström et al., 2011; Yearley, 2000). All in all, the important lesson that stems from this stream of work is that the politics of public engagement is always context dependent (see also Yearley, 2011).

When it comes to critically assessing questions over validation with policy relevant modelling, the exemplary case in the STS literature has been, however, that of climate change policies and the use of GCMs. If this characteristic type of modelling has played any role in climate change policy<sup>49</sup>, this has happened despite the amount of uncertainty, or so the argument often goes (Edwards, 2010, 2001, 1996a; Van der Sluijs et al., 1998). GCMs offer relatively crude and imprecise outputs but still policy-makers can find them useful, as demonstrated by Van der Sluijs et al. (1998). They discuss the stability of the numerical temperature range regarding the notion of 'climate sensitivity'. The concept relates to the potential global temperature change if the concentration of carbon dioxide is instantly doubled. Climate sensitivity is

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<sup>49</sup> Cf. Edwards (1996a) account of Brunner's (1996) belief that climate change models are largely irrelevant to policy (see also Dessai and Hulme, 2004; Rayner et al., 2005).

obtained using the outputs of several General Circulation Models. Each CGM provides a numerical value<sup>50</sup> and a range of values is produced – or, better, socially constructed – by simply combining the estimates from all the different models available. According to Van der Sluijs et al., based on assessments up to 1995 a climate sensitivity range of 1.5°C-4.5°C remained strikingly unchanged over two decades despite improvements in the GCMs – e.g. much better spatial resolution (see Edwards, 2010). And yet, they claim that this was not a problem for the different actors involved. Van der Sluijs et al. conclude that the wide interpretative flexibility and ambiguity of the 1.5°C-4.5°C temperature range suited both the scientific community at large and the policy-makers and sustained it. By and large, the consensus around the climate sensitivity to CO<sub>2</sub> worked as a useful ‘anchoring device’ that helped to hold together – despite the uncertainty – a number of social worlds, not only science and policy, but also specific scientific disciplines. Rather than accuracy, it was usefulness that mattered. This has been partly echoed by other accounts as well, which stress the role of climate change models to help to create a public space for consensus building on global climate issues, or more precisely, an epistemic community (Edwards, 2001, 1996a, after Haas).

Indeed, modelling has been long portrayed as having a central influence on policy for reasons more to do with communicative and organisational usefulness than with the imprint of real-looking representations. Scientifically sound solutions to policy problems often involve actors coming from different social worlds and computer models can facilitate social learning (Clark et al., 2001) and perform as ‘social glue’ (Shackley, 1997), which is also consistent with the understanding of models as boundary objects (Shackley and Gough, 2002; Halffman, 2003; Millo and MacKenzie, 2007, 2009; Van Egmond and Bal, 2011; Van Egmond and Zeiss, 2010). In the same line as with climate change modelling, the computer model known as ‘Regional Air Pollution Information and Simulation’ (RAINS), used in the

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<sup>50</sup> Without considering explicitly the uncertainty because GCMs are deterministic models.

building up of the Convention on Long-Range Transboundary Air Pollution, is often presented as an exemplary case of how models can ‘mediate’ in the policy process. It facilitated the establishment of international political consensus on the problem of acidification in Europe and to get actual emission reduction policies on their way (Van Daalen et al., 2002).

On the whole, in this literature the key point is that the coupling of models to policy is generally mild and mediated. In some of these cases modelling can offer a slightly stronger coupling to policy as agenda setting. This is for instance the case when they raise awareness of certain policy salient issues. An exemplary case of this was the earth system model ‘Word3’ that informed ‘The Limits to Growth’ report to the Club of Rome in 1972 (Meadows et al., 1972). Although the modelling has been rendered as overambitious and unrealistic by its very authors (Meadows et al., 2004), and had no actual capacity to inform social intervention directly, the fact is that it had political impact at the time in terms of a heuristic guide to complex phenomena, as pointed out by Edwards:

It is certainly true that through its models, popular books, meetings, and person-to-person canvassing of politicians, the Club succeeded in communicating, to both a broad public and a policy elite, its two basic heuristics: (a) that exponential growth (especially in population) cannot continue unchecked, and (b) that the world should be viewed as a set of interlocking systems that cannot be understood successfully or managed piecemeal (1996a:154).

In other words, the model became an important vehicle in bringing high on the national and international political agendas the new problem perception of a global ecological crisis anchored in the question of limits to economic and population growth. More recently in time modelling played a similar role in feeding the environmental policy discourse with the problem of acidification (Hajer, 1995) and climate change (Edwards, 1996b). There are indeed relevant computer models in both areas – and beyond – that have been conceived to support policy as tools for

comparison of policy scenarios in a heuristic manner – known as integrated assessment models.

Either with a strong coupling to policy or with a milder and mediated one, I shall argue that the different sets of STS work on the role of computer modelling for policy generally show a certain ‘narrative bias’. Stewart and Williams (2005; see also Williams, 2006) coined this expression to point at how particular schools of thought often come along with the same classic stories, dramatic repertoires, dilemmas, pitfalls, problem diagnoses, solutions and champions. It follows that too many times the role of the modelling turns out to be the central focus at the expense of other elements of policy process<sup>51</sup>.

Indeed, STS commentators may have not questioned enough how much they may be reproducing the visions (after Russell and Williams, 2002) of the modellers that ascribe to their tools a centric position in the policy process (see Dessai and Hulme, 2004), or become enchanted by the often ‘seductive tactics’ of the modelling, as Shackley and Darier (1998) self-diagnose. Furthermore, the reality is that models are the product of a small subset of the scientific community – namely the modellers – and that the outcomes of the modelling do rarely inform the broad policy-process (Rayner et al., 2005; Shackley and Darier, 1998).

Notably, there have been attempts to overcome the narrative bias in the social constructivist outlook on modelling for policy. Using the exemplary case of climate change modelling, Yearley (2009) points at the IPCC to argue that it is not just the representations produced with the modelling that count, it is how the (co-)production of the IPCC reports is organised between scientists and diplomats – beginning with who gets to write the reports. Meanwhile, in discussing the social construction of the

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<sup>51</sup> As hinted by Gough et al. (1998).

GCMs Wynne (2010) also moves away from the centrality of GCM predictions to policy and puts the emphasis on the scientifically framed meanings of the climate change problem and what this brings to the politics of world-making (see also Demeritt, 2001). Yet, beside some exceptions like these, the usual take of the STS social constructivists on the role of computer modelling for policy-making is marked by the assumption of centrality.

In moving away from the narrative bias, I shall argue that modelling for policy is about building and being part of the sociotechnical infrastructure that allows evidence-based policy-decisions. In this sense, the discussion on the use of the notion of boundary object can provide an insightful parallelism. In 2010 Susan Leigh Star published a response to the myriad of scholarly work that has misused the notion by focusing on the ‘object’ and failing to capture the whole infrastructure that boundary objects are part of. Instead of singling out objects that enable interpretative flexibility across a social diversity – potentially anything – the attention has to shift to the entire organic infrastructure of different types of boundary objects through which social groups cooperate as a result of information and organisational needs. Thus, the role of modelling for policy is mediated by this infrastructure and then it is key to understand how the infrastructure works for policy and how modelling works for the infrastructure. Furthermore, depending on context, actors can be pragmatic about epistemic uncertainties and this has practical consequences over the usefulness of the modelling for the infrastructure and ultimately for policy (Van Hemert, 2013). Along the same lines, Millo and Mackenzie (2007, 2009; see also MacKenzie, 2003) provide a thorough account of how financial risk management models prevail as part of an informational and organisational infrastructure regardless of their lack of representational accuracy<sup>52</sup>.

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<sup>52</sup> Getting ahead of myself, in the social studies of fisheries literature there are also relevant examples of how modelling can entrench for organisational-political reasons despite the problems with uncertainty (Holm, 2007; Holm and Nielsen, 2004). Indeed, it is worth noticing the cross-fertilisation between these two strands in the social studies of finance and in the social studies of fisheries.

As the argument goes, the STS approach to the role of modelling for policy needs to be a ‘decentred’ one in order to foreground context and infrastructure. Speaking of which, let us then move to another element of the framing of EU fisheries management decisions that I will address in the main case study, the so-called integrated impacts assessments.

### **2.5.2 Integrated impact assessments in Policy Studies and beyond**

Decision-makers need to become informed about all the likely consequences of their policies and regulations (Owens, 2007). IIAs can be defined as formal procedures for dealing with multifaceted policy plans, using knowledge from various scientific disciplines and stakeholders, so that those comprehensive insights can be made available to decision makers (Parson, 1995; Rotmans, 1998; Van der Sluijs, 2002a). They represent the aim to address complex societal issues through an interdisciplinary and more often transdisciplinary process (Gough et al., 1998; Rotmans and Van Asselt, 2001). Following Shackley and Gough, “[i]ntegrated [impact] [a]ssessment is currently seen as the method of choice for bringing large scale scientific analysis into policy frameworks” (2002:1). They can be regarded as part of the ‘scientisation of politics’ (Hoppe, 1999); appealing to policy-makers because they expect the provision of hard numbers against which alternative policy options can be evaluated. This despite that IIAs tend to be based on changeable and contested understanding, especially regarding the socio-economic assumptions in the modelling used to produced those hard numbers. In other words, they are unlikely to hide the subjective character of at least some of the choices made in their design<sup>53</sup> (Parson, 1997; Parson and Fisher-Vanden, 1997; Schneider, 1997; Van der Sluijs, 2002b; Van de Sluijs et al., 2005).

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<sup>53</sup> A common issue is the challenge of balancing simplicity and complexity (Rotmans and Dowlatabadi, 1998; Shackley and Gough, 2002; Rotmans and Van Asselt, 2000).

The field of Policy Studies has paid heed to the role of IIAs in EU policy-making. Nonetheless, it is worthwhile to recall that there is a significant split between two schools of thought with regard to how IIAs actually work – or should work. Some positivist scholars voice the gap between the techno-rationalist ideal behind IIAs described above and the actual practice in the EU (Renda, 2006). It is not that the positivists generally believe that IIAs could actually work by the book, but actual practice is always assessed against the techno-rational template (Hertin et al., 2009a). For instance, IIAs can be said to foster communication processes between experts and stakeholders to improve the quality of the information. However, one line of critique addressing a deviation from the technical-rational ideal refers to the involvement of stakeholders brought into the production of knowledge for policy decisions; it tends to hit back in the sense that, “greater openness also allows greater scrutiny of factual issues in the assessments leading to the paradoxical situation where openness produces more criticism” (Bäcklund, 2009:1082). Hence, the implication for Bäcklund (2009; see also Wilkinson et al., 2005) is that, while pursuing better evidence, IIAs may inherently weaken the knowledge base, favouring the political use of the IIA exercise by pressure groups to bail their pre-existing interests. By and large, neutrality and objectivity, which in theory make IIAs amenable for improving EU regulation, are not what IIAs deliver in practice.

Yet, other post-positivist scholars argue that intrinsic shortcomings are not the cause of a mismatch between the IIA’s theory and actual implementation. It is the narrow rationalist theory underpinning IIAs that is inadequate in the first place – they are not politically neutral devices to provide more rational decision-making. This group of policy analysts have taken a look at the empirical practice to describe how IIAs work away from this textbook ideal (Radaelli, 2005, 2004; Hertin et al., 2009a; Hertin et al., 2009b; Jacob et al., 2008; Owens et al., 2004). As Radaelli and Meuwese (2008; see also Radaelli and De Francesco, 2007) claim, IIAs can instead become from tools

for political control of the bureaucracy<sup>54</sup> to tick-the-box exercises with no influence on policy choices<sup>55</sup>.

Hertin et al. (2009a) advocate keeping both the technical-rational vision and the post-positivist's account as partially useful descriptors of the role of IIAs<sup>56</sup>. Similarly, Owens et al. (2004; see also Jacob et al., 2008; Farrell et al., 2001) stress that it is often the by-product of IIAs to open up politics for deliberation and learning across the different constituencies of the policy process; "[These] technical procedures have, [if only] as an unintended effect, provided important apertures for deliberation and learning" (Owens, 2004:1950). Hertin et al. (2009b) extend the argument further as follows:

While guidance documents tend to describe [IIAs] in terms of a rational problem solving process, the reality observed in practice often corresponds to a process in which the objectives and problems are continuously reframed and re-interpreted (2009b:418).

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<sup>54</sup> Indeed, interest groups and Member States' governments have resorted to IIAs to limit the amount of regulation coming from Brussels or at least reshape the formulation of proposals, sometimes by producing counter-IIAs:

The argument is simple. Given a politically controversial issue of who has to exercise control on law-making in the EU (with the member states, the European Parliament and business lobbies pushing for more control and the Commission entrenched around the Treaty right to initiate legislation and all that follows), progress on fundamental reforms [...] is impossible. Theoretical work on IA in the US is clear on the fact that what is at stake with this instrument is the political control of the bureaucracy [...] Yet in Europe this notion has always been ditched in the formal discourse and the rhetoric surrounding Better Regulation. However, the substance of the debate is political control both in Brussels and in Washington. But this is too hot to handle (Radaelli and Meuwese, 2008:4).

<sup>55</sup> According to Radaelli and De Francesco (2007), while the economists have long discussed whether the IIA instrument helps a more efficient regulation, research shows that regulations that should not have passed the net benefit test have been adopted quite frequently.

<sup>56</sup> It is noteworthy, however, that the insights from the post-positivists have had very little impact in the general understanding of IIAs. The technical-rational linear-sequential model has been the dominant way of thinking drawing on the authority of the scientific method to inform policy (Hertin et al., 2009a).



This is consistent with a vision of IIAs as formal procedures, but where interaction and flexibility become key aspects, and lesser emphasis on technical analysis in practice facilitates conversation between political actors thus enabling stakeholders to have a say in the policy process (Meuwese, 2008). As the argument goes, what IIAs are able to provide is an answer to the question of who has authority in the policy process (Radaelli and Meuwese, 2008).

The question of authority also links to a discussion about the often-challenging integration of expertise behind IIA exercises, an issue where STS scholars have stepped in. For instance, Skarstad (2008) describes the disciplinary-based controversies within an expert group of toxicologists and nutritionists in charge of an integrated impact assessment over how healthy eating salmon is in Norway. Skarstad not only argues that such an exercise was not a neutral political tool but shows that the IIA worked as a space where the discussion could shift from the production of knowledge for policy to the re-framing of the policy problem in a way that could better reconcile and accommodate the distribution of authority – i.e. as a space for alignment between toxicologists and nutritionists. Often the struggles are between natural sciences and the social sciences (and humanities), as Redclift (1998) argues for the case of advice on climate change; the climate problem is framed in terms of natural sciences<sup>57</sup>, and particularly in terms of atmospheric physics, and only takes into account social and human considerations some way down the road (Shackley and Wynne, 1995a). For some commentators, however, the danger with IIAs is that they may open for economic concerns and not pay too much attention to environmental considerations, especially given that IIAs are often conducted under severe time pressures and with incomplete information (Owens, 2007).

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<sup>57</sup> Notably, Rayner and Malone (1998) question whether the problem of climate change might have been framed in the wrong way and in fact should be seen as a problem of humans making the wrong choices in everyday life – rather than a climate that is changing.

When it comes to how individual IIAs can influence the decision-making process an often-cited example is the science–policy interaction that took place under the context of the United Nations Economic Commission for Europe’s Convention for Long Range Transboundary Air Pollution – CLRTAP (Castells and Ravetz, 2001; Farrell and Keating, 2005; Gough et al., 1998; Lidskog and Sundqvist, 2011; Sundqvist et al., 2002; Tuinstra et al., 1999). The IIA exercise was an attempt to develop an analytical framework for a regional cost-benefit and cost-effectiveness analysis of policy options to control air pollution<sup>58</sup> (Tuinstra et al., 2006). The case illuminates that it is not easy to draw a sharp line between scientific and policy making activities in an assessment process. Tuinstra et al. argue that the knowledge produced within the CLRTAP integrated assessment process and the institutional setting in which this knowledge production took place cannot be separated from each other. Neither can the scientists’ or policy makers’ roles as actors in such processes always be precisely defined. There is always negotiation over the division of labour.

Generally speaking, the science–policy interaction that took place within CLRTAP is portrayed as a success story in the literature, although the reasons vary. Looking at context, Gough et al. (1998) observe the important role that the framework played for countries to provide their own data and the consensus-based style of working. Farrell and Keating (2005) emphasise the flexibility in adjusting the use of the RAINS model to the increasingly complex effects-based protocols, stressing the high

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<sup>58</sup> Several models at various research centres in Europe were under development according to Tuinstra et al.:

For the actual preparations of the negotiations three different models, ASAM (Imperial College, London), CASM (Stockholm Environment Institute (SEI), York) and RAINS (International Institute for Applied Systems Analysis, Luxemburg, Austria) were used in the Task Force to calculate different scenarios with different policy options, which were presented to the Working Group on Strategies. As far as possible the models were run with the same data, provided by the parties to the Convention. The output of the model runs took the form of levels of emission reductions per country and the resulting percentages of ecosystems protected. The final calculations that served as a starting point for the negotiations of the second sulphur protocol, the Oslo protocol (1994) were done by the RAINS model, developed at IIASA. The other models were being used for comparison (2006:354).

levels of trust within which the assessments were conducted. Ishii (2011) highlights the role played by what he calls ‘diplomatic science’, that is, the creation of specific scientific categories for diplomacy settings. For others, some of the key factors for this success are to be found inside the modelling such as, *inter alia*, multiple scenarios presenting different policy options; accessibility to the model, which could be run on a personal computer; and cost effectiveness analysis – which does not involve monetisation of effects – rather than cost benefit analysis (Hordijk, 1991; Maas et al., 2004; Tuinstra et al., 1999).

By and large, what the literature illuminates that there is no single linear or mechanistic relationship as such between the IIA exercise on the one side and the policy process on the other. Let us now move on with the elements of the framing EU fisheries policy decisions directly from the world of fisheries management.

### **2.5.3 The social studies of fisheries governance and its instruments**

This thesis looks in particular at the technologies of politics that take part in the solution of problems of governance in EU fisheries management<sup>59</sup>. The two previous subsections have dealt with somehow horizontal technologies in the sense that one can find them in all kinds of framing efforts to enable evidence-based policy decisions in the EU and elsewhere. I will now refer to the framing efforts directly

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<sup>59</sup> To clarify the terminology, and as flagged by Nielsen and Holm (2007), one should distinguish between fisheries resource management and fisheries management, as follows:

While resource management [e.g. a fish stock] for obvious reasons is an important concern in modern fisheries, it should not be mistaken for the only one, and it should not be treated as identical with the more generic concept of fisheries management. We can have fisheries management without resource management, for instance when the fishing pressure generated is small compared to the productivity of the resource. Resource management only becomes imperative when fishing pressures approach or exceed the self-generating capacity of the resource (2007:671).

In the cases at stake in this thesis clearly fisheries resource management can be assimilated to fisheries management and I will use the latter throughout the text.

linked with the conduits of fisheries policy; that is, to the machinery of fisheries management.

Fisheries management in the EU stands out as an area that presents exciting challenges given the diversity of actors, the fragmentation of the EU fisheries polity, and the organisation and role of mandated science. Notably, some commentators claim that fisheries management is perhaps the most science-dependent sector in the EU (Daw and Gray, 2005). Whether the top-spot is deserved or not, what is interesting is to understand how it is perfectly coherent for fisheries policy to be – in parallel – probably one of the most politicised areas in the whole of EU policy.

The bottom line is that under the Common Fisheries Policy decisions have been based on bargaining games between EU member states (Da Condeição-Heldt, 2012). In order to solve this problem the solution has consisted of bringing in more science and associated ‘anti-political’ (after Barry, 2002) instruments to depoliticise fisheries decisions<sup>60</sup>. Despite this the EU has not performed as a successful fisheries manager (Cardinale and Svedäng, 2008; Daw and Gray, 2005; Wilson, 2009). The reasons deserve a thorough exposition and I will dedicate the whole of Chapter 4 to make the case in full. Meanwhile, in this subsection I will address the general rationale of fisheries management and the use of sound regulatory machinery for the framing or depoliticisation of fisheries management decisions.

The coastal waters of the North Atlantic have been fished for centuries, and for the most part of the time, under free open access and none or little control over the fishermen (Bavington, 2009; Johnsen et al., 2009). The crisis picture with conflicts

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<sup>60</sup> Depoliticisation in this context can be assimilated into the protection of fisheries policy rational decisions from the negotiation skills of the national political authorities that sit on the Council of the European Union – also known as Council of Ministers – in the same way that the fish needed to be protected from the effective catching capacity of the fishermen and the fishermen from bursty fluctuations in the amount of fish.

over fishing rights and over-exploitation of fish stocks is a relatively recent phenomena that has followed ultimately from growth of the fishing fleets and the improvements in the technologies for harvesting right after WWII (Johnsen et al., 2009). Affairs such as the armed dispute between Iceland and the UK for cod catches in the 1970s and the depletion of the Norwegian spring-spawning herring stock during the 1960s opened for a new regulatory regime introducing rights-based fisheries management to avoid the race for fish. As followed, the international agreements reached towards the end of the 1970s led to a much more central role of the states in regulating fisheries. By and large, in order to prevent the next crisis, the first thing that was done was to safeguard access to the pool of resources by making them public property in the hands of the coastal state – a leviathan (Holm and Nielsen, 2007; Jentoft et al., 1998) with authority to bend the selfish rational fisherman's behaviour leading to overexploitation and his own ruin.

The influence of the prescriptive seminal article by Garrett Hardin (1968), the 'Tragedy of the Commons', was in fact notorious at the time (Jentoft, 2004; Johnsen et al., 2009). It was on the basis of Hardin's thesis<sup>61</sup> as well as the thinking of other fisheries economists<sup>62</sup> that the cornerstone of modern fisheries management was

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<sup>61</sup> In his widely read article published in 'Science' in 1968 Hardin puts forward the proposition that lies behind exploiting resources under conditions of open access and common property: rational economic behaviour of individual actors using without any imposed limitations a common resource leads inevitably to the depletion of the resource and the ruin of all the actors depending on it in one way or the other. In other words, the best way not to preserve a common pool of resources and to impoverish the commoners – like for instance the fish stocks and the fishermen after them that Hardin used as one paradigmatic example – is to allow right of free access to the commonality. This is because while it is perfectly rational for the individual fisherman to increase the fishing effort to catch the fish before anybody else, this is irrational for the whole group of users of the resource. Self-regulation is not a way out because of the problem of collective action when rational individuals can opt for free riding – i.e. sharing the costs of exploitation collectively while cashing out the benefits individually. Hardin and the economists conclude that it is only some form of property rights – publicly owned by the state or privately owned by the fishermen – that relieve the fisherman from the inevitable tragedy (Jentoft, 2004).

<sup>62</sup> Prior to Hardin, in 1954 the Canadian fisheries economist Scott Gordon was the first to point out that the common property and free access regime operating at the fishing grounds at the time allowed too much competition – given the fast development of the fishing industry the fishermen could only become poorer because the costs of finding and catching were bound to escalate with the race for the fish. The solution according to Scott Gordon was to shift to a different regime with private or state

grounded. Thus, ever since the 1970s the driving rationale in fisheries policy has been that is feasible to manage fisheries in a ‘control and command’ rational way to achieve certain social objectives such as economic yield maximisation or, more recently, ecological and social sustainability (Degnbol, 2004).

Since the advent of state-led fisheries management most governments have approached the regulation of the rights to fish for the catching industry through annual quotas. Each year the fleets should only catch so many tons out of the total estimates of the size of the fish stocks. Putting it short for now, the biologists were ready to advise annually the so-called ‘total allowable catches’<sup>63</sup> (TACs), which actually enabled in an effective way a division of labour between science and policy by allowing the framing of fisheries management problems in a technical fashion, at

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ownership where the fishermen could find incentives to behave rationally over the long-term period (Bavington, 2009).

<sup>63</sup> As Bavington describes it:

After one hundred years of state-sponsored fisheries research, by the 1960s scientists had finally provided governments with knowledge of how to profitably control landing fluctuations in cod. By representing fish as statistical populations and developing quantitative bio-economic models, they were able to predict future biomass abundance under a variety of fishing regimes and give advice to governments on the most optimal fishing levels for given biological or economic goals. National governments, however, were unable to implement fisheries management because they did not have the right under international law to take ownership of fish populations beyond their small territorial sea zone that only extended to three miles offshore (2009:109).

The development of the stock population dynamics modelling in the late 1950s thanks to the work of Beverton and Holt represented a pinnacle in fisheries biology (Degnbol, 2004). Drawing on this knowledge, fisheries managers could control a fishery by monitoring the mortality caused by the fishing activity. Indeed, the population models illuminated a counterintuitive notion – that less fishing should lead to maximum sustainable yields over the long-term (Bavington, 2009). Notably, the biologists excluded the behaviour of the fishermen from their models; the actual fishermen were simply expected to follow suit and come in line with what the biologist’s models suggested, but instead they raced for the fish as voiced by Gordon (Holm, 1996). Without ownership of the stocks, claimed Gordon, there is no incentive for the fishermen to act in a biologically rational way and bring their fishing effort in line to achieve maximum sustainable yields, or even more reasonable maximum economic yields in the long-term. Only ownership would produce the collective economically rational fishing behaviour assumed in the models to exist in the human nature of each and every fisherman. Collective economically rational fishing action is untenable if access to the commons is freely available and its property shared.

least more than other alternatives<sup>64</sup> (Holm and Nielsen, 2004). In this context, the ‘bread and butter’ for fisheries science – and particularly for fisheries biologists – has been to provide policy-makers with technical instruments to implement their rationality<sup>65</sup> (Degnbol, 2004).

Indeed, the structures for the production of knowledge and evidence-based decisions in modern rational fisheries management have introduced ‘mechanistic’<sup>66</sup> (Burns and Stalker, 1994[1961]) relationships between the fish, the fishermen and the policy-makers, that is, between the so-called ‘system-to-be-managed’ and ‘the management system’<sup>67</sup> (Kooiman et al., 2005; Jentoft, 2007; Berkes, 2010). Furthermore, modern fisheries management has approached the system-to-be-managed in reductionistic

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<sup>64</sup> Drawing on ideals of objectivity and independence, the fisheries advisory framework has been therefore structured in principle so that fisheries policy and science can remain separate. Scientists assess the size of the stocks as well as the total tons of fish that the fleets should be allowed to catch and policy-makers decide on where to place the limits, that is, the actual size of the quotas. Holm and Nielsen (2004) describe how the establishment of quotas in tons of fish was central to that division of labour. The alternative to the time-consuming calculation of fish stock numbers based on information about the cohorts was to produce estimates on the basis of how much effort it takes vessels to catch the fish. The former option was favoured in the international policy arena back in the mid 1970s not because of any epistemic merits in terms of representing better the amount of fish in the sea but because quotas in tons could offer a workable and stable solution to the problem of setting the quotas in international negotiations, granting in principle a neat split between science advising on TACs and policy-makers deciding on quotas. All in all, the unit of fishing effort was much more ‘slippery’ and thus difficult to standardise across countries with fleets using different fishing gears than a ton of fish.

<sup>65</sup> As put by Jentoft in the following terms:

[F]isheries research institutions relieve management agencies from some of the political pressure they would otherwise be exposed to. Knowledge validated by the institutions of science makes the state more confident and less vulnerable [in principle] when controversial management measures are implemented (2005:149).

<sup>66</sup> Burns and Stalker put forward the following characterisation of the term:

Mechanistic systems (namely “bureaucracies”) define [the individual’s] functions, together with the methods responsibilities, and powers appropriate to them; in other words, however, this means that boundaries are set. That is to say, in being told what [the individual] has to attend to, and how, he is also told what he does not have to bother with, what is not his affair, what is not expected of him, what he can post elsewhere as the responsibility of others (1994 [1961]:103, emphasis in the original).

<sup>67</sup> Modern fisheries management assumes that there is demarcation and mutual interaction between a socially constructed governing system and a natural system-to-be-governed (Kooiman et al., 2005; Jentoft, 2007; Berkes, 2010).

fashion by intervening in the conduct of fisheries through fishing quotas that allocate fishing rights for individual fish stocks (Johnsen et al., 2009). And while TACs and quotas can perform successfully towards distributing fishing rights, very often they have not worked well enough towards preserving the fish<sup>68</sup> (Holden, 1994; Holm and Nielsen, 2004). This is evidenced by remarkable collapses such as that of the Newfoundland cod stocks in 1992, where the utter inadequacy of the stock assessments produced by the scientists under pressure from the Canadian government<sup>69</sup> despite the alerts coming from the (alienated) inshore fishermen failed conservation dramatically<sup>70</sup> (see Finlayson, 1994).

Fisheries science has been notably ‘shaky’ to say the least, particularly around the turn of the 21<sup>st</sup> century. There were indeed three major issues affecting all across the production and uptake of fisheries scientific advice according to Degnbol (2004). First, there is the problem of uncertainty in the scientific assessments for policy. In response to this issue, fisheries policy-makers adopted the precautionary principle – which arrived formally to this domain in 1995 with the publication of the ‘FAO Code of Conduct for Responsible Fisheries’ – as a fundamental basis for fisheries management. Putting it simply, it became accepted that no regulatory action should be delayed by uncertainty when there is risk of serious or irreversible harm to the marine environment. One of the consequences of the introduction of the precautionary approach was that estimations of the degree of uncertainty in the scientific advice had to be made more explicit. This development matched a parallel shift in fisheries science with the introduction of stochastic modelling; casting uncertainty as probability of occurrence and using stochastic modelling to showcase

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<sup>68</sup> For an exception see Holm’s (2007; see also Pálsson, 1998) account on how the Norwegian Sea was transformed into an ‘aquarium’, an ideal setting for fisheries managers where it was possible to exert cybernetic-like control over the fish stocks resources. Further below I will describe how Holm and other scholars accounted for a special kindness of these resources in order for the aquarium to work. It is also relevant to bear in mind here the contextual evidence of the particularly ‘cold’ (after Callon, 1998) waters of the Norwegian Sea, where revolutions can even be ‘invisible’ (Holm, 2001).

<sup>69</sup> With overestimations of the stock’s size by as much as 100%, thus portraying the idea of a very successful management of the stock (Finlayson, 1994).

<sup>70</sup> Stocks fell to 1% of their past size during the heyday in the 1960s (Finlayson, 1994).



it has ever since been a common approach used by fisheries scientists<sup>71</sup>. Second, in EU fisheries and other geographies, managers and scientists were setting up fishing mortality objectives without the involvement of stakeholders, thereby affecting the legitimacy of such goals. Often the perception of stakeholders was that fisheries scientific advisors were too frequently subdued to policy (Finlayson, 1994). In this sense, since the early 1990s there was increasing awareness of the need to include legitimacy as an important objective in its own right within the dominant rationale (see also Gray, 2005; Symes, 2006; Wilson et al., 2003). Third, quotas for individual stocks required a specific type of scientific advice, a highly institutionalised production of knowledge where some disciplines were more dominant than others.

Notably, biology has been the discipline that fisheries managers in the North Atlantic have turned to in their demand for formal advice to set the quotas under the rights-based regime – certainly more than to economists or social scientists. And according to Degnbol et al. (2006) this has invited ‘tunnel vision’ – i.e. advice confined to the theoretical assumptions of the dominant discipline. In response to the dominance of the stock assessment biologists, the disciplines left out have traditionally come up with their own universalistic ‘technical fixes’ to tackle once-and-for-all the problems of fisheries management beyond the shaky scientific advice (Degnbol et al., 2006; Nielsen and Holm, 2007). Degnbol et al. note the three competing – but not necessarily mutually exclusive – discourses for the reform of the dominant biological rationale in fisheries management. One avenue is that of a market for fishing quotas or fishing rights. Advocates of this rationale – largely the economists and advocates of Hardin in general – argue that fishermen are much more keen to adopt sustainable

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<sup>71</sup> Notably, the arrival of precautionary management and stochastic modelling was nothing more than the uptake of uncertainties and risk assessment into the existing management rationality and institutional framework. As in other contexts, the assumption was that uncertainty could be overcome through bigger computer models to include even more processes and the collection of more data to feed these models (Degnbol, 2004). Indeed, some fisheries scientists believed that bigger and more sophisticated modelling was the way to move forward, including more parameters – e.g. accounting for interactions between different stocks – in the hope that this way they could faithfully mimic what goes on in the sea (McGlade, 1999).

practices if they own the resource, that is, if they can buy and sell their rights to fish. To make it work all that is needed is to implement a system of ‘individual transferrable quotas’ (ITQs). The second possibility is ecological fixing, embraced by marine ecologists and environmental NGOs alike. A key instrument for these actors is the so-called Marine Protected Areas (MPA). These are areas of special environmental protection where fishing and other human activities are restricted or prohibited. The idea is that MPAs can operate as ecological sanctuaries with no pressure over the spawning stocks and recruits thereby increasing fish abundance within and facilitating the spill-over into neighbouring areas. In general, MPAs have not been able to live up to expectations in those places where implemented. A third avenue is that of co-management with the fishing industry, or ‘community-based management’ (CBM). Its advocates – largely sociologists and anthropologists – claim (normatively) that involving the users of the resource in its management is what delivers sustainable fisheries rather than the other way round. It is notorious the strong empathy towards the fishermen as well as their experience-based knowledge of the seas that rests at the core of social science work on fisheries in a somehow ‘anti-Hardinist’ response to the dominant thinking informing actual practice within managers (Holm, 1999; see also Nordstrand and Holm, 2009). Put bluntly, while fisheries policy-makers have embraced Hardin’s thesis at face value and have closed the commons, the social scientists have denounced on empirical grounds that policy-makers could have done better than treating the fishermen as universal selfish rational hunters, thus disregarding their local context, their local knowledge and their potential contribution to the management of the fisheries (Jentoft, 2004; McCay and Acheson 1987; Wilson et al., 2003). In fact, as the argument is sometimes put, involving the fishermen in the management of the resource should help to reduce the complexity of fisheries regulation by moving away from top-down regulatory architectures and micro-management (Johnsen, 2013; see also Carter, 2013b; Gray and Hatchard, 2007). Moreover, fishermen would be more supportive of fisheries

science as well if their contextualised contributions were taken seriously into account (Holm, 2003) and the advisory process became more transparent<sup>72</sup>.

Drawing on the problem of disciplinary tunnel visions, scholars have raised concerns over the limitations of the fisheries policy instruments in the face of the complex environment that they are meant to deal with (Berkes, 2010; Jentoft, 2007; Kooiman et al., 2005). Following from governance theory, they point out that the limits to how a fishery should be governable come from the management system's inability to cope with the properties of the issues to be governed in a holistic manner. They suggest better adjustment between the management system and the system-to-be-governed in order to integrate environmental, humanistic and societal elements into fisheries management in response to a system-to-be-governed that they see as a comprehensive socio-ecological system. Yet, rather than any radical reforms, as Jentoft and Mikalsen (2004) put it, fisheries managers often look for new instruments that are close enough to existing ones. Pretty much more of the same or incremental change seems to be the standard response to challenges.

For other scholars in the social studies of fisheries this is not necessarily a problem. Holistic perspectives miss the mark when claiming that governability depends on how compatible the attributes of the management institutions and instruments – the management system – are with the system-to-be-governed, as well as on the ability

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<sup>72</sup> Yet some social scientists alert of the so-called 'paradox of transparency' (Wilson, 2009). Marine scientists have enjoyed a lot of pre-eminence in the advice to fisheries management at the EU level and elsewhere, as opposed to the fishing industry. In the view of these social scientists, this is justified by virtue of the scientific method, which provides the best foundation for regulating collective action in the 'EU pond'. The bottom line is that fisheries scientists are always driven by internal transparency across their research community, even if this can bring inevitably some opacity to the production of advice by the very nature of their science – e.g. with the use of sophisticated computer models. Ironically, so the argument goes in Wilson (2009), they cannot give away their internal transparency for the sake of less opaqueness towards non-experts without giving up the quality of their science – because of the skills and training needed to assess it. Notably, these requests for openness to stakeholders are part of a broader picture where marine scientists are increasingly dissatisfied with the conditions they have to face in their work (Kraak et al., 2010; Schwach et al., 2007; Wilson and Hegland, 2005).

to produce feedback from the latter in the form of all-inclusive scientific representations. The governability of fisheries, they argue, depends instead on the success of the constitutive co-production of the management system and the objects-to-be-managed by the different actors involved; such success relies on the actors – beginning with the fishermen – either domesticating or accepting this co-production (see Johnsen, 2013; Johnsen et al., 2009).

Looking at the infrastructure for the production of knowledge and evidence-based decisions in modern rational fisheries management, which these scholars characteristically refer to as the ‘TAC Machine’ (Holm, 2007; Holm and Nielsen, 2004), they argue that the demarcation of the governing system and the objects-to-be-governed is artificial. In actual fact, the sociotechnical infrastructure co-produces the object-to-be-managed, what Holm (2007; see also Johnsen, 2013) calls the cyborg fish stock upon which management is acted<sup>73</sup>. While single fish stocks were once considered to be living natural objects, under the framing of the TAC Machine they have been transformed into a socio-natural order through computer models, procedures and negotiations. The TAC Machine has turned the natural fish into new entities that can be calculated; the wild and intractable fish from the sea is replaced by a manageable fish inhabiting computer screens and quota proposals (Bavington, 2009; Holm, 2007; Johnsen, 2013; Nielsen, 2008).

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<sup>73</sup> Holm spells out the full idea as follows:

[In the Norwegian Sea] we find the reconstruction of the fish, from a wild creature of the sea into a complex, cyborglike, scaled, and modeled entity – a resource fit for management. You may be excused here if you here think I am referring simply to the invention of mathematical models of the behavior of exploited fish stocks. But such models are only the tip of the iceberg, drawing attention away from the much more basic work of setting up measurement procedures, sampling standards, and networks of data collection and refinement that allow the variables in the models to be filled with realistic-looking numbers. It is by the way of such complex networks of measurement systems, as much as the abstract models they tie into, that the fish can be captured and transported into computers and made to produce scenarios for fisheries to come. And when the fish finally has made its way up to this iceberg-tip of abstract modeling, it starts working its way back down again, now in the form of quota propositions, from negotiation table to negotiation table, becoming increasingly harder, more enforceable, and more subdivided, until it ends up as a catch allowance or quota for a particular fishing vessel (2007:238).

As I shall detail in Chapter 4, the TAC Machine, with its assessments of individual fish stocks, has been particularly contested in the EU because of two major externalities, namely failing stocks' conservation and the alienation of the fishing industry. Yet, despite all the scrutiny and criticism of the TACs and quotas by stakeholders, the TAC Machine has survived the passage of time by virtue of being politically useful for the distribution of fishing rights between EU members (Holm and Nielsen, 2004).

All in all, the key to understanding how fisheries are made governable is to focus on the instruments that are used to produce governable objects and actions (Johnsen et al., 2009; Johnsen, 2013). That is, to concentrate on the sociotechnical framing and what makes it fragile or resilient. By and large, how a framing works depends on how useful or problematic is it publicly sanctioned rather than on the technical sophistication to match or be compatible with the complexity perceived in the real world. Indeed, often the central aspect to governability is whether the actors-to-be-framed can shape the set-up of the framing through political work (see Griffin, 2013; Johnsen, 2013).

## **2.6 *Firming up the issues of inquiry***

The assembling of the nautical chart for this thesis is now complete. In the previous sections I have offered the conceptual coordinates that will guide the lines of inquiry. It makes sense to end the chapter with a firmed up version of the research issues. In addition, I will also expose how other tangential issues will come to the fore in the course of the thesis.

In general terms, I am interested in how computer modelling really counts for (fisheries) policy-making. Yet, in tackling this line, three specific issues emerge. Drawing on the cross-fertilisation of the social studies of fisheries and the social studies of finance (Holm, 2007; Holm and Nielsen, 2004, 2007; MacKenzie, 2003;

Millo and MacKenzie, 2009), one issue is whether usefulness for the sociotechnical framing under a given context can make the modelling prevail even if the modelling limitations with uncertainty are exposed under that context. A second issue is how to define this usefulness – for instance in the context of integrated assessment modelling, which is precisely designed to be useful to policy without having to deliver accurate predictions. The third issue has to do with contending the presumption that modelling alone becomes central to the policy process and its outcomes, which I shall argue is a narrative bias.

Yet there is more to come though. Taking the analysis at the level of the policy process, with the troika of instruments that support the framing of decisions as the unit of inquiry, will allow me to see afar. There are a number of tangential issues that I will come across in this research journey – what I have already referred to as the ‘by-catch’ of my enterprise. The weightiest one – deserving a section of its own in the concluding chapter – is how fisheries are made governable, which will be followed with suggestions for further research on whether other EU sectors may be made governable in the same way as the fisheries one is. Being a little bit more precise, I shall claim that this thesis can contribute not only to the body of knowledge about the role of modelling for policy, but also to a still incipient policy analysis of how the EU is indeed practised and to a line of work where knowledge-practices and technologies of governing receive deserved attention as contributors to, “the constant re-ordering of Europe as an institutional and political entity which we may otherwise read about in the daily newspapers” (Waterton and Wynne, 2004:88; see also Barry, 2001; Callon, 2004; Carter, 2013b; Carter and Lawn, forthcoming; Carter and Smith, 2008; Halpern, 2010; Jasanoff, 2005; MacKenzie, 2009; Rowell and Mangelot, 2011; Smith, 2013; Sundqvist and Letell, 2005).

The research goals are set. However, before moving on to the empirical chapters I shall detail first some of the methodological choices that inform this thesis in the following chapter.



## 3 Methodological considerations

### 3.1 Introduction

If the previous chapter offered a ‘nautical chart’, in this chapter I will offer methodological insights into how I ‘spread the net far and wide’ during the course of my research. It shall read as a stand-alone *ex-post* justification piece on the different choices that I made along the research journey, which started formally in September 2005 on a full-time basis and, as of January 2008, continued on a part-time basis before seeing a long interruption of more than three years between August 2009 and December 2012, all for professional reasons.

It was between the second half of 2006 and the first half of 2007 when I conducted my fieldwork across a number of European countries, and a great part of this chapter will be dedicated to expose the fine-grained details of how it worked out. Nonetheless, before offering any insights into the fieldwork, I shall give indications of how I framed the empirical undertakings in the first place, which demands some attentive spelling out. Central to my strategies in the field was a well-known dictum in STS, that of ‘following the actors’. This is the somehow bold and apparently unsophisticated proposition of the forefathers of actor-network theory to approach empirical work. Yet, the very short maxim in fact represents the practical translation of a whole theory about how to study problems pragmatically – as Latour (1999, 2005) has ‘recalled’ ANT. The bottom line goes like this. Rather than setting out with a clear set of conceptual categories to try out on the actors, what is paramount is to learn from them first-hand their problematisations. Meanwhile, it is also idiosyncratic in this tradition to consider that following the actors encompasses more than just simply the human beings involved in the problematisation of affairs and the quest for solutions; it is the ‘socio-technical ensembles’, with all the non-human



mediators that help to sustain them, that need following, or in other words, foregrounding<sup>74</sup> (Bijker, 1995; Latour, 2005).

Following the actors is probably better understood in the context of a case study than in the abstract. In this sense, Latour illuminates the role of the analyst in characteristic vivid terms for the case of the mad cow disease that struck in Europe around the mid-1990s:

For instance, you begin with a T-bone steak on your plate and you end up in the laboratory of a protein specialist showing you the tertiary structure of the now infamous prion, one of the possible causes of the so-called ‘mad cow’ disease. But in the mean time you have visited European Commission bureaucracies, the cattle farmers’ unions, quite a few hospitals, and participated in a lot of scientific meetings (Barry, 2001). In brief, you have traced a network – a network, to prevent any objection from people not familiar with our use of the word, being not a thing in the world but the path traced by the researcher (2003:36).

This is quote that I bore in mind during my fieldwork as a useful reminder of how to go about. It helpfully illuminates another important point for how I approached the empirical work. It is not simply following actors at any given point in time that can bring rewards to the analyst, it has to be done on the spot(s) in real-time, while actors engage in the co-production of natural and social orders – like the prion. It does not matter much that following the actors may sound a bit of a challenging and confusing mandate because of the so many potential actors for the analyst to follow when crisis hits. The network is not something that lies out in the real world waiting to be ‘represented’, captured in a holistic manner<sup>75</sup>, but the journey of the analyst in her

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<sup>74</sup> There has been sharp ‘humanistic’ criticism of the principle of general (agential) symmetry between human and not human actors introduced by ANT from the start (Bloor, 1999; Collins and Yearley, 1992). However, following Latour (2005, 1999; see also Yearley, 2005), I am not taking ANT for what is not, namely a sociological theory. Therefore, I do not find that symmetry is a discussion for this thesis, where I mostly conflate doing ANT with doing fieldwork of a particular kind.

<sup>75</sup> Cf. Jasanoff’s (2004) criticism of the early ANT accounts.

strategy of making-sense of the world. Of course, those journeys can always spark criticism as biased descriptions of a singular and unique situation and, overall, fail to convince. Indeed, especially in the early days of ANT, actor-network followers showed a bias towards managerial actors, scientists and engineers as heroes of innovation (Haraway, 1991; Star and Griesemer, 1989). Yet, as later expressed by Latour (2005), there is always the risk for any account of reading simply as ‘artificial’. But the difference is not between artificial and accurate, but between writing ‘good’ accounts – both artificial and convincingly accurate – or ‘bad’ accounts that do not manage to convince because they come across as too artificial. As follows, any considerations of method should be oriented towards producing artificial but convincing accounts<sup>76</sup>, and this is where following the actors is somewhere to start.

That was indeed the case for me. Given that at the beginning of my fieldwork I did not enjoy the comforts of a structured framework nor in-depth knowledge of what I was intending to study, following the actors and learning (*tabula rasa*) directly from them as I went along was my only chance of sounding one day remotely convincing – beginning with the very informants in the field! Following the actors allowed me to turn a handicap into an advantage. It was by having my eyes wide-open from the outset that I became immediately intrigued by the redefinition and redistribution of existing actor’s roles or the creation and mobilisation of new ones, a common thread under the context of two case studies that I will introduce below. Yet, while following actors somehow maximises the opportunities to produce meaningful situated research, it is also important to bear in mind where some of the limitations of the approach lie.

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<sup>76</sup> There are accounts that are artificial and well known to be inaccurate but nonetheless exemplarily useful. Winner (1980, 1986) is one of them.

In this respect, it is noteworthy that following the actors may resonate with other qualitative approaches in the social sciences. For instance, ‘grounded theory’ (Glaser and Strauss, 1967), which in its original formulation demanded the analyst to be free from any theoretical pre-conceptions, later to be reformulated in terms of openness when it comes to social terminology, not to impose any particular one on the informants (Strauss and Corbin, 1990). Reading ANT as a methodological resource – rather than a theory – may lead to a certain degree of conflation between the two approaches at the time of entering the field. The differences become quite significant at the output end nonetheless. Grounded theory aims high, at generating theory (Pidgeon, 1996). Meanwhile, following the actors aims at generating descriptive accounts (Latour, 2005). And in this sense, ANT gets much more often conflated with ethnomethodology – Latour (2005) himself claims that ANT is half ethnomethodology, half semiotics. Indeed, ANT borrows from ethnomethodology the very idea of diving into the specific situation under study to develop competence in the everyday routines of the social actors and account for them like an insider (see Blok, 2010). Ethnomethodologists need this immersion to pay heed to the observable practices and materials that perform the orderliness of social life – including scientific mundane life<sup>77</sup> – as well as to how those performances become validated by actors within the particular social milieu (see Yearley, 2005). As follows, both approaches are able to produce singular empirical descriptions for equally unique cases. Yet, it is precisely in the difficulty to derive more informative conclusions than the ‘just thisness’ (Yearley, 2005, citing Lynch, 1993) of a particular practice or material where the problem with both approaches start (Yearley, 2005; see also Blok, 2010). In the case of ANT, Blok observes somehow critically that reluctance towards far-reaching generalisations should not distract us from the need of social scientific

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<sup>77</sup> For instance, what standing at the blackboard to offer a mathematical proof means to mathematicians regardless whether the actual mathematics are a universal truth or a social convention (see Livingston, 1999) – the latter being, where the interests of social constructivists lie (Yearley, 2005).

concepts allowing scales of abstraction between the particular and the general<sup>78</sup>. By and large, “abstraction is what enables our knowledge of particulars to be gradually compared, allowing us to search for patterns of social relations across dynamic time-spaces” (Blok, 2010:67). This is indeed the reason I have chosen the co-production framework for doing conceptual work<sup>79</sup>.

All in all, it should be made clear that I will not offer in this chapter a contribution to knowledge in what concerns questions of method because there is already a well-developed sense in the field of STS of what it means to follow the actors with ethnographic eyes. My approach to fieldwork was business-as-usual, running on the shoulders of this tradition. As follows, I will account for what it was a creative process of cutting my way across the field<sup>80</sup> and provide substantial *ex-post* justification for the choices so that the chapter contributes to build a (hopefully) convincing set of arguments in this thesis. In so doing, in the remaining three sections of the chapter I will inform the practical methodological choices and concerns behind the design of my research. In §3.2 I will deal with issues concerning the research design. In §3.3 I will provide details about my conduct of the fieldwork. And in §3.4 I will describe how I faced the analysis and reflect on the mutual influence between the research and the research participants.

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<sup>78</sup> To illustrate vividly this point I shall turn to Blok’s reference to Borges’s short fictional story ‘Funes the Memorious’ (1998 [1942]), “a man capable of remembering every single minute detail of every single split-second sensorial experience he ever encounters, but sadly incapable of any type of forgetting, and thus also of any type of abstraction or generalization (a distinction, incidentally, also made by Borges!)” (2010:66).

<sup>79</sup> Some clarification is needed though. Latour (1992) already talks of the co-production of nature and society but it is in the abstraction and ultimately generalisation of cultural patterns where the co-production framework conceived by Jasanoff offers its added value: “[The co-production framework] makes apparent deep cultural regularities, to the extent that it explains the contingencies or durability of particular socio-technical formations, it also allows us to imagine the pathways by which change could conceivably occur” (2004:42).

<sup>80</sup> There is an ever-inspiring Spanish poem by Antonio Machado that starts as follows: “Wanderer, your footsteps are the road, and nothing more; wanderer, there is no road, the road is made by walking” (see Graige, 1978:n.a.).

## **3.2 Research design**

### **3.2.1 Research strategy: ‘Making sense’**

In the lengthy introduction to this chapter I have already made clear that the reasoning process in this thesis comes after the combination of the pragmatic approach of learning *tabula rasa* from the actors and of the co-production framework for the conceptual work that enables a whole exercise of making-sense and looking for patterns of regularity – as opposed to fact-making<sup>81</sup>. It is by knitting together the evidence gathered from the actors with some conceptual work that the analyst may be able to conform a convincing narrative of ‘why something is the way it is’ (Bertilsson, 2004). This is a style of reasoning commonly known as ‘abduction’<sup>82</sup> (see Blaikie, 2007, after C.S. Peirce), which in very simple terms consists of seeing – somehow unexpectedly – what one did not see before (Bertilsson, 2004).

Notably, abductions may help as much good detective work as the act of discovery in science (Bertilsson, 2004). In other words, Sherlock Holmes’s characteristic work in situations of uncertainty constitutes a valid research strategy for forming new situated knowledges that, “may or may not assume roles in future social conversations” (Blok, 2010:66). Consistent with this way of reasoning, Jasanoff makes clear that the co-production framework is about bringing “the society’s collective habits of interpreting and ordering experience within the perimeter of social inquiry” (Jasanoff, 2004:27). By and large, much like in other social practices, social science inquiry may rely on a variety of abductions, in which the analyst forms situational explanatory guesses for puzzling observations by drawing on her own judgment (Blok, 2010).

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<sup>81</sup> Quoting Jasanoff, “[the aim of co-production] is to make available resources for thinking systematically about the processes of sense-making through which human beings come to grips with worlds in which science and technology have become permanent fixtures” (2004:38).

<sup>82</sup> It is noteworthy that the notion of abduction originally referred to the generation of hypotheses in the natural sciences (Blaikie, 2007, after C.S. Peirce).

Yet it is important to bear in mind, just as ethnomethodologists argue, that the more embedded the analyst is within a given scientific culture, the more she takes for granted and the lesser opportunities for puzzlement – thus the importance of starting with following the actors in ethnomethodological style.

### **3.2.2 Research method: Case studies**

At the level of methods this thesis is generally aligned with the STS tradition of conducting qualitative case studies and, more in particular, with most post-constructivist studies and their aim to study the co-production of science and social order through ethnographic case studies that are multi-sited, materially sensitive, with no micro-macro distinctions and where timing matters, since co-production is easier to observe in controversies around the proliferation of uncertainties and conflicting values (Blok, 2010; Jasanoff, 2004).

Yet, the case study method needs to be carefully approached, if only because it continues to be considered second best among social science methods (Thomas, 2010; Yin, 2009). Its ambiguous characterisation hardly comes to help. For instance, a popular definition of the case study method is that of Yin:

An empirical inquiry that investigates a contemporary phenomenon in depth within its real-life context, especially when the boundaries between phenomenon and context are not clearly evident and multiple sources of evidence are used (2009[1984]:18).

The whole point is, in theory, that single cases studies should be able to invest the singular, situated, real-life setting with a sense of generality, or at least to enable some sort of abstractions of a broader social phenomena. Yet this has to depend necessarily on what is the case ‘a case of’ in the first place. The question has sparked lively discussions in the case studies research literature (see for instance Flyvbjerg, 2006; Ragin, 1992).

Flyvbjerg argues that one can of course generalise after observing a certain frequency with some phenomenon, but this is not the only way to generalise. Single case studies, when strategically selected for being critical cases, can be ideal for falsifying or verifying scientific propositions, which is another pathway to producing general knowledge (see also Ragin, 1992). Flyvbjerg identifies critical cases as those ‘most likely’ or ‘least likely’ scenarios where the proposition works that can, respectively, lead to falsify or confirm propositions and hypotheses. In addition, he points out that a purely descriptive case study without any ambition to produce generalisations can certainly be of value – we learn when moving from case to case so, even if knowledge cannot be formally generalised, this does not exclude it from the process of knowledge accumulation in a given field or in a society. As Flyvbjerg follows, case studies can illuminate that, what looked in the first place ‘white’ in a situated context, turns out to be surprisingly ‘black’ on closer examination – note the hallmark of the abduction process. Indeed, some scholars argue that, “[there is] a need to move away from the expectations of generalizable knowledge that go alongside inductive process [and] move toward the “*exemplary knowledge*” of abduction and phronesis” (Thomas, 2010:578, emphasis in the original).

As follows, validation of case studies comes, not through reference to a theory or generalised knowledge but with producing convincing narratives that other people can connect to (Thomas, 2010; see also Abbott, 1992).

As one might expect, the case study method has triggered similar debates within the field of STS that are worth detailing. Case studies have formed an important part of this epistemic landscape of the field since the 1970s (see Beaulieu et al., 2007; see also Jensen, 2013). On the one hand, STS has been able to turn the weakness of ethnographic case studies into an advantage. If there is an area where ‘strategic’ (Marcus, 1995) case studies have been effectively used for deconstructing and problematising claims of generality that must be STS, for instance with the scientific method (Beaulieu et al., 2007; see also Yearley, 2005, especially for his account of

Bloor (1991[1976]) and the ‘hard case’ of mathematics). On the other hand, the idea of learning from case to case is recurrent in the field but has not produced theoretical illumination, leading to repeated discontent in STS with the accumulation of case studies (see Jensen, 2013). Wyatt and Balmer miss, for instance, more justification of the like: “What is this a case study of? What does it add to our understanding of different concepts? Interesting as the story itself may be, how does it contribute to discussions of anything beyond itself?” (2007:619). Geels (2007) puts emphasis on the need for mid-range concepts that can bridge particular cases with high-level theories. Meanwhile, Beaulieu et al. (2007), criticise the excess of cases studies that are simple application of theories in order to exemplify already established analytical points. However, “how can they be used to inspire other work, without explicitly or implicitly falling into a universalizing fallacy?” (Beaulieu et al., 2007:673). Along the same lines, Jensen expresses concern in that, “the risk I have in mind is that applying the same theories to evermore cases lead to diminishing interest, with the consequences that the dishes presently on offer in STS too often have a bland taste” (Jensen, 2013:11). Indeed, Jensen (see also Jasanoff, 2010a) advocates finding in case studies opportunities for lateral learning, for the integration and mutual translation of knowledge across fields. This is as well how I understand the case studies presented in this thesis. I have not aimed at applying theory, falsifying theory or generating theory but at landing new or under-researched vistas, lines of inquiry crossing over to other fields, and some unexpected ‘by-catches’.

### **3.2.3 Case studies selection**

Bearing all this in mind, this subsection informs the selection of the two case studies that I shall present in the empirical chapters. I will provide much more contextual details in due course. For the time being, in this subsection I shall focus on what aspects convinced me in the first place that they were singular cases worth the time and effort in the expectancy of making relevant contributions to knowledge. The first one is what I refer to as the primary or main case, while the second one is more of a complementary or subsidiary case.



The setting for both case studies is the area of public affairs concerning the management of fisheries, as I have already informed. Fisheries management must be one of the most fruitful areas for the study of the role of modelling for policy. At first sight at least, modelling is used to estimate the number of fish in the sea and decisions are meant to be based on this information. It certainly looks as if modelling was central to the policy process as provider of compelling representations. Yet it is also well-known across European publics, particularly in those countries around the North Sea, that computer modelling in fisheries, the scientific calculations of how many fish swim in the sea, face considerable uncertainty, and that the stakes are high, with the fishing industry and the environmental NGOs often raising their voices in the policy processes.

The main case study is indeed about a fishery in the North Sea. The North Sea is one of the most complex fishing areas a fisheries manager in Brussels can face. There are 23 commercial fish stocks assessed by ICES and it is surrounded by seven countries – six EU Member States plus Norway – and their respective fishing fleets. It is not difficult to see why the North Sea is considered the ‘laboratory’ for fisheries policy innovation in the EU (see Griffin, 2010), a characterisation that resonates straightaway with STS laboratory studies. To the point that the North Sea is the setting for which the EU fisheries management instruments are normally designed for in the first place, as well as where they are normally tested. The particular North Sea fishery that I chose for the case study is the mixed fishery of plaice and sole. This fishery poses a great challenge to managers in Brussels because of the asymmetry of the two overlapping fish stocks – normally, what the sole stocks demand from policy-makers is different to what the plaice stocks do. As I shall explain, the North Sea flatfish fishery was a test-bed for a number of policy instruments introduced in EU fisheries policy in the year of 2002. As part of the policy innovation there was some new, more comprehensive modelling that was still in the making. That such work-in-progress modelling could be of use directly in the policy process around the management of the flatfish fishery was what intrigued me first when I knew about this fishery. As I shall describe, by and large, the

management of the flatfish fishery in the North Sea had many elements of a real-life experiment aiming at the mechanisation of fisheries policy decisions. It was an opportunity to observe what kind of role new innovative modelling played for policy when uncertainty was assumed to be considerably high. Moreover, the timing was good and it was feasible for me to follow the actors within the policy process in real-time. All in all, this case study seemed of real interest in order to explore how experimental modelling was still useful in practice regardless of the sheer uncertainty and whether the modelling had indeed a direct influence in policy or mediated through the sociotechnical framing.

Meanwhile, the second case study looks at the North East Arctic cod fishery in the Barents Sea, shared by Norway and Russia. The fieldwork – which was based only on interviews as opposed to multi-sited participatory observations and interviews for the main case – was all conducted from Norway. Initially, I went to Norway mainly with the idea of getting some (anthropological) distance from Brussels and the North Sea case and grant myself the possibility of observing unforeseen matters that could help me strengthen the sense-making of the North Sea case study; in other words, to be able to see things that I had not realised before. Norway and the NEA cod fishery were relevant choices for a number of reasons. First of all, if the EU was considered a bad fisheries manager, Norway was regarded as the benchmark<sup>83</sup>. Second, the NEA cod fishery was also a Russian business, so its management had to be shared between the two very different countries. In this respect, the NEA cod fishery was the most iconic fishery in the Barents Sea and where new management instruments – including modelling – were often first introduced. By and large, it was another singular case study like the North Sea one; in both settings existed the ambition of

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<sup>83</sup> As evidenced, for instance, by the fact that a EU delegation, with representatives not only from the European Commission but also from stakeholders' groups, visited Tromsø during my spell in the city to conduct interviews in June 2007. My interview on 21 June 2007 at the Norwegian Fishermen's Sales Organisation, which handles the first-hand sale of fish from the fishermen to the fishing industry, was cut short by the arrival of this delegation from the EU eager to learn first-hand about Norway's best practices. So much for taking distance from Brussels!

depoliticising and mechanising fisheries management decisions. Chances were that I could unearth some kind of pattern of regularity across the otherwise two very different cases. All in all, this second case was conceived to help me to abstract and strengthen the findings of the main case study, as well as the convincing-ness of the research account as a whole.

Before moving on with the description of the fieldwork conducted for these two case studies it is necessary to make one more remark. In the case of the main study about the North Sea flatfish fishery, the empirical work consisted of interviews and participatory observation across a number of countries – Belgium, Denmark, Germany, the Netherlands and the United Kingdom – and locales – laboratories, fishing ports, governmental offices, *ad hoc* meetings and so on. Following the actors is an obvious way of conducting such multi-sited ethnography, as the quote from Latour (2003) above illuminates (see also Marcus, 1995). Yet, following the actors is a much more affordable endeavour in a single-sited, perhaps even enclosed, environment like the lab than in a multi-sited, perhaps even multi-country, setting that cuts across dichotomies between the micro and the macro, the local and the global. In single-site ethnography an intensive focus on the actors is the rule. It is the intimate encounters in a specific space that grants ethnographic studies their edge (Beaulieu et al., 2007); they promise accounts of a niche world as it really is – without adulteration from theory (Jensen, 2013). By contrast, in a multi-sited ethnography, the analyst is by definition mobile and adaptive to circumstances, taking mostly opportunistic-unexpected trajectories in the tracing of a cultural formation across a diversity of locales. It is a mapping of a terrain without the holistic representation ambitions of the *mis-en-scene* single site; thus a different kind of ethnography (Marcus, 1995; see also Hess, 2001). This is an important remark to make ahead of the empirical chapters. Time now to detail how I carried out the fieldwork for these two cases.

### 3.3 Fieldwork

It is important to start by noting that fieldwork is a dynamic process where the choices of method aforementioned become entangled with all sort of practicalities – for which justification can only come *ex-post* (Blok, 2010). Further, in a multi-sited ethnography not all the empirical sources are of the same intensity and quality, as pointed out by Marcus (1995). Within a case study there are time constraints, budgetary limitations, logistical issues and so on. The same applies to a multi-case study, not all the case studies can be carried out with the same strength.

Language can be another aspect affecting quality according to Marcus. In the two cases at stake, although the research was conducted in English-as-a-second-language, the fact is that using the so-called ‘European’ English was more an advantage than an actual problem. With both ends of the conversation speaking this more straightforward dialect of English, interaction could take place on an equal footing language wise. It has to be said that only on one occasion I observed a meeting that was completely in Dutch and needed the help from another informant to translate on the spot the answers of a fisherman during an interview that I conducted straight after that meeting<sup>84</sup>.

Data collection in the field spanned approximately from June 2006 till July 2007 with occasional exchanges of emails to follow up developments until February 2008. In order to facilitate access by relatively short journeys to the majority of informants for the individual interviews, I spent a 6-month research visit in the Netherlands<sup>85</sup> and 3-month research visit in Norway<sup>86</sup> during the course of 2007. Notably, ‘staying

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<sup>84</sup> It was a one-off exception and I considered that this degree of informal translation should suffice for the purposes of this thesis.

<sup>85</sup> At Maastricht University funded by the PRIME Network of Excellence’s ‘PhD circulation scheme’.

<sup>86</sup> At the Norwegian University of Science and Technology funded by the ‘Norwegian Research Council’ under the Bilateral Culture Agreements between Norway and Spain.

local’ helped me as well to build a rapport with informants in the two countries, as I shall describe below.

### **3.3.1 Preparing for the field**

A valuable insight from the recent turn in the sociology of knowledge looking at the study of expertise is that the STS analyst cannot expect to achieve quality in her fieldwork without developing some degree of ‘interactional expertise’ (Collins and Evans, 2002) in the area of knowledge that her research touches upon<sup>87</sup>. Therefore, at the very beginning the analyst’s efforts should aim at gaining ground from ‘no expertise’; the degree of expertise with which the fieldworker usually sets out is insufficient to conduct quasi-participatory fieldwork – at least providing she had no previous involvement in that particular field.

There are various strategies that the researcher has at hand in these situations. One way is to conduct a pilot study if there is time and financial means available. Yet with problems that are happening in real-time – and, therefore, which are not that easy to control – the pilot case study is not such an option. The alternative in these cases is to move to the field and try to speed up the learning curve by conducting thorough documentary analysis in parallel<sup>88</sup> to the interviews and participant

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<sup>87</sup> Expertise that should improve as the fieldwork progresses until a learning curve is reached.

<sup>88</sup> Notably, there are some critical aspects to the analysis of documentary evidence (Platt, 1981). For instance, any evidence has to be assessed in terms of the source of the document and motives behind it. Another element is to develop some criteria of what can be put aside in the face of a sheer volume of documented material. In my case, the documentary analysis was an absolutely key part of the following of the actors’ strategy. Actors in both case studies produced a large amount of reports and other relevant written accounts and I had to be extremely careful with putting things aside in order to avoid overlooking relevant connections and potential informants. By and large, I analysed a large number of documents from public bodies, associations and NGOs, including scientific papers, commissioned reports, policy briefs, position papers, working papers, official letters, press releases, articles in the media, minutes of meetings, and conference programmes and proceedings. Only those that I cite directly are reflected in the list of references of this thesis. Yet I would actively use a large amount of documents during the interviews to help my understanding of issues and increase the rapport with the interviewees.

observations. Three events at the start of the fieldwork were key learning opportunities in this respect:

- *International Council for the Exploration of the Sea (ICES) Symposium on Fisheries Management Strategies (Galway, June 2006, open event)*

Policy makers from the European Commission, EU Member States, and other coastal states, fisheries biologists, social scientists, fishing industry representatives and environmental NGOs spokespersons met for four long days to discuss the ‘state of the art’ of fisheries management strategies. At this event I identified the potential for a case study of the introduction of a long-term management plan for the North Sea flatfish fishery in the context of the then still fresh reforms of the Common Fisheries Policy. I also established contacts that, not only allowed me to be kindly invited as an observer to the following event on this list, but also helped throughout my fieldwork<sup>89</sup> and put me in touch with other informants – the so-called ‘snowballing’ technique (Goodman, 1961).

- *UK Focus Group for the FP6 Project ‘Operational Evaluation Tools for Fisheries Management Options (EFIMAS)’ (York, August 2006, by invitation)*

Notably, the EFIMAS project, funded by the European Commission through the 6<sup>th</sup> Framework Programme, aimed at developing scenario simulation models so that fisheries managers could explore the effects of different choices as if the modelling were a ‘flight simulator’. In order to take into account the views from fisheries managers and stakeholders while the modelling was still being designed, some focus groups were organised around Europe. I was invited to observe the one that took

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<sup>89</sup> With some informants I managed to build a standing relationship throughout the fieldwork. For example, one of my informants kindly invested in a number of meetings at his place training me in fisheries science and modelling – for instance, he provided me with a simple model in Excel so that I could play around with key concepts. At some point I even passed a small test – which reminded me to a modest extent of Harry Collins and his trial to prove intermediary expertise with the gravitational waves research community (see Collins and Evans, 2002).

place in the UK. It was my first encounter with the kind of modelling approach<sup>90</sup> that I would later follow as part of the main case study.

- *ICES – Annual Science Conference (Maastricht, Sept 2006, open event)*

This is the largest annual event for fisheries biologists in the North Atlantic. Back in 2006, it was a good opportunity to become more familiarised with vocabularies and cutting edge research in fisheries science. Policymakers and stakeholders were also present in some of the sessions related to fisheries management. This is where I learned about the integrated impact assessment for the new management proposals for the North Sea flatfish fishery. This IIA exercise ended up being a very important focal point in the main case study. Overall, it was also an excellent chance to continue with informal chats in the corridors in order to scope my case study in the North Sea. Furthermore, in one of the multiple parallel sessions I heard for the first time about the North East Arctic cod fishery in the Barents Sea, which I followed afterwards for my second case study.

### **3.3.2 Gaining access**

In general access was not the problem that I thought it could have been at the beginning of my fieldwork. I was able to learn quickly from these events and the documentary analysis and I used that knowledge strategically in the field. Recurring to snowballing from the key contacts made at those events was important to go beyond some of the spokespersons, but the policy process was well reported and I needed to move quickly not to miss important elements. Thus, as part of the strategy of following the actors in the policy process, I also approached informants boldly; sometimes in the context of an event – as I shall detail below – sometimes by email<sup>91</sup>

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<sup>90</sup> Which I will introduce in Chapter 5 as ‘Management Strategy Evaluations’ (MSEs) or, more generally, as ‘Integrated Assessment Modelling’ (IAM).

<sup>91</sup> I used emails to set appointments for interviews. This seemed more appropriate than formal letters. Moreover, I could personalise them very quickly for each informant and they could reply to me promptly as well. Given that I was following the policy process in real-time this aspect was key. Emails were also a good way to follow up interviews.

explaining that I was following the policy process of the North Sea flatfish fishery and showing that I had good insiders' knowledge and was interested in having a conversation.

Notably, I received consistent reassurance from several informants that it was very good timing to be around researching EU fisheries and the policy process for the North Sea flatfish fishery in particular. In other words, social actors welcomed the fact that there was someone doing academic research on an innovative case at the right time. This indeed contributed to granting me access. In addition, the fact that I showed up repeatedly at various events in different countries contributed to demonstrating that I was trying to do a serious job and also helped me. In a way, a rather similar thing happened in Norway as well. While getting access in Norway was not a difficult thing once I explained I was conducting research for my PhD, the fact that I had already conducted research on a North Sea EU fishery was generally seen with positive interest. By and large, it helped me to show that I was intending to do a professional job.

There was only one particular event where I experienced problems with access and this was the first meeting of the integrated impact assessment exercise for the management of the flatfish fishery in the North Sea – formally known as SCEGA-SGRST-06-05. I only learned about this meeting taking place at the end of September 2006, that is, a few days beforehand and, due to the short notice and the fact that the quota of observers had been already met, I could not get an invitation to attend. However, the working group responsible for conducting the integrated impact assessment produced a very detailed report and, together with in depth interviews of most of the attendees to this meeting, I managed to perform some 'virtual witnessing' of the event.



### **3.3.3 Data collection: Interviews**

Planning and conducting interviews took a large amount of the time that I was away for fieldwork. For the main case study of the North Sea flatfish fishery I conducted semi-structured interviews with 39 informants – 29 male and 10 female – spread across Belgium (7 informants), Denmark (6 informants), Germany (1 informant<sup>92</sup>), the Netherlands (20 informants<sup>93</sup>) and the UK (5 informants). The interviewees represented all major actor roles in EU fisheries management, ranging from DG-FISH and Member State officials (8 informants) to fisheries biologists (12 informants), economists (7 informants), social scientists (2 informants), fishing industry representatives (4 informants), NGOs representatives (2 informants<sup>94</sup>) and fishermen (4 informants). In a few cases I held several interviews and multiple informal exchanges with the same (key) informant(s). The majority of the interviews were face-to-face – only on two occasions I used the telephone for a first interview<sup>95</sup>, as it was not possible to organise a face-to-face interview. The interviews were tape-recorded under verbal informed consent that I would use the material for my PhD with a guarantee of anonymity promised – removing the possibility of distinguishing real names. One informant asked not to be recorded but allowed me to take notes and I treated these in the same way as the transcripts from the recorded interviews.

For the supplementary case study of the North East Arctic cod fishery in the Barents Sea I conducted interviews with 23 informants – 18 male and 5 female – spread

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<sup>92</sup> Interview conducted over the phone.

<sup>93</sup> One interview was conducted over the phone.

<sup>94</sup> Of which one was a representative of the fishing communities' networks and the other a representative of an environmental NGO. I was unsuccessful in my attempts to interview other environmental NGO representatives. It turned out that there were only two environmental NGOs actively involved in the daily work of the NSRAC at the time of research. As I shall describe, they were generally understaffed to take part in a very demanding organisation such as the NSRAC, with its numerous meetings and heavy workload (see Degnbol and Wilson, 2008). On the other hand, the environmental NGOs produced large amounts of documentation to disseminate their views, which I consulted extensively.

<sup>95</sup> Notably, eye contact had been already established at previous events. The advantage of telephone interviews are that the costs are lower and, while you tend to get shorter answers it is still possible to maintain a dialogical setting. I also used telephone interviews sometimes to follow up previous face-to-face interviews.

across Norway (20 informants) and Russia<sup>96</sup> (2 informants). In principle, I tried to cover more or less the same spectrum of actors than in the main case study. In this sense, the interviews targeted the following actor-roles when it came to the Norwegian side: government officials (2 informants), fisheries biologists (7 informants), economists (5 informants), fishing industry representatives (4 informants), fishermen (1 informant) and environmental NGOs (1 informant). Except for this last interview, which was conducted over the phone because it was not possible to organise it otherwise within the time available<sup>97</sup>, the rest of the interviews were face-to-face. Meanwhile, the two Russian informants were fisheries biologists and I used telephone interviewing with them as well<sup>98</sup>. As described for the main study, in a few cases I held several interviews and informal exchanges with the same (key) informant(s), which was always very useful in order to see how my interactional expertise was progressing. The interviews were all also tape-recorded under verbal informed consent that I would use the material for my PhD and a guarantee of anonymity promised.

When it comes to the process, by and large, a semi-structured interview should not be seen simply as asking questions and waiting for the responses to come. It is a much more dynamic face-to-face interaction between interviewee and interviewer under a given context (Kvale, 1996; MacKenzie, 2005). For this reason, it is essential to create some rapport. Interviews, which were mostly held at the interviewees' work

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<sup>96</sup> This is a clear asymmetry but for purposes of contrasting, of spotting unforeseen issues with my main case study, I believed that the research conducted from Norway could be enough. Should I have had more time, financial resources and a less tight word-limit I would have probably researched the Russian views on the Barents Sea NEA cod fishery because of a genuine interest in the case. Multi-sited ethnographies bring these kinds of choices to the researchers' table. As Marcus (1995) puts it, one cannot expect the same intensity all across.

<sup>97</sup> As in the case of the NSRAC, the representatives from the environmental NGOs were only a few and generally overworked. I could only reach this informant over the phone. Despite no previous face-to-face contact, it worked out as a very fruitful conversation nonetheless.

<sup>98</sup> I had not established previous contact with them either but again this did not make a substantial difference judging from the result, which were long and insightful conversations. The logistics for these two interviews to be face-to-face were difficult and not worth the effort, especially in the light of the previous footnote (107).

or private addresses to make it less disturbing for them, would typically start with me introducing my research and interdisciplinary background briefly, followed with questions about the background of the interviewees and the organisation they worked for. Then, trying to show I really knew ‘my fisheries’<sup>99</sup>, I would often enter into real conversations<sup>100</sup>. I would finish by asking for advice on further people to contact, how to better approach an elusive potential informant, meetings to attend or further issues I should be aware of. Interviews were on average one hour long but several times went on for more than three hours when conversations really flowed<sup>101</sup>. I tried to allow the actors the possibility of taking the conversation to what they felt was relevant and important. I also adapted to their circumstances – for instance, if I needed to co-walk with my interviewee because that was the only way to have enough time for a real conversation then I would do it.

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<sup>99</sup> An expression that I heard quite a few times in the field.

<sup>100</sup> Collins addresses the clues of interactional expertise as follows:

[H]ow do you know you have acquired interactional expertise in a subject? The answer is in the ways you can interact. As a sociologist of science you essay research on a new specialism and you initially understand neither the banter nor the technical terms. After a painful period, if you are lucky you begin to pick up on the inferences in others’ conversations and eventually you begin to be able to join in. One day a respondent might say in response to one of your technical queries ‘I had not thought about that’, and pause before giving you an answer. When this stage is reached respondents will start to be happy to talk to you about physics and even respond generously and with consideration to your critical comments. Eventually people will become interested in what you know, not as a scientist in your own right, but as a person who is able to convey the scientific thoughts and activities of others. If you’ve just come from visiting scientist X you may be able to tell scientist Y something of the science that X is doing [...] What were once ‘interviews’ then become ‘conversations’ that can be interesting and occasionally even useful to both parties. What also happens in a conversation is that by occasionally anticipating a point your partner is about to make you can speed things along. You might also verbally fill in some gaps that might otherwise be forgotten. You can recognize jokes, irony and when you are having your leg pulled (though, in the nature of things, interactional competence does not allow you to recognize lies). When you get good at it you can even take the devil’s advocate position in respect of some scientific controversy and maintain it well enough to make your conversational partner think hard (2004:104).

I can certainly recognise this account in the experience of my own fieldwork. I could lead conversations towards certain topics using the technical vocabulary appropriately (see also Bell, 2010[1987]).

<sup>101</sup> When I was offered the opportunity for a longer than usual conversation I absolutely took it as these were excellent opportunities to run my interpretations past these informants and see if my understandings of the issues at stake were solid. Holding multiple interviews with the same informant was also a way of checking my interpretation of things (see Burgess, 1984).

In general, I came across very reflexive informants who gave me much more than ‘the line’. As I mentioned earlier, social actors in both case studies welcomed my research interests and timing, which genuinely helped me to get ‘accepted’ (Bell, 2010[1987]) as well as the quality of the conversations.

I did some of the transcripts immediately after the interviews when I considered that a given interview had been particularly insightful and would help to prepare for other interviews. This also allowed me to see what kind of questions worked best so that I could refine my list of general questions<sup>102</sup>. Yet, the majority of the transcripts were done after the fieldwork<sup>103</sup> with the help of professional services – which I would then check thoroughly. It was only at that point when I had all my data transcribed and checked that I started with the proper analysis phase. I will come back to this point below.

### **3.3.4 Data collection: Participant observations**

Following with the asymmetry between the two cases, the collection of data for the Barents Sea case was based exclusively on interviews in different parts of Norway running parallel to the policy process in real-time while for the North Sea case the data collection in real-time also involved participant observations across multiple

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<sup>102</sup> Indeed, for every interview I readjusted my questions based on the analysis of documentary evidence and notes of what I had already learned from previous interviews and participant observations. What I mean here is more refining aspects about general lines of inquiry. For instance, at the very beginning of my fieldwork I used to ask a lot of questions centred on the modelling but, as the fieldwork went on, I used more questions about the policy process – and the modelling would simply come up in the conversation. Before I realised, the sociotechnical became the actual unit of inquiry during my fieldwork.

<sup>103</sup> Traditional ethnographies ‘by the book’ stress that the collection of data and the analysis should be an iterative process (see Hammersley & Atkinson, 1995; see also Hess, 2001). However, I did not proceed this way for two reasons. The very practical one is that, in the case of my multi-sited ethnography across different countries, time that I could have spent doing the analysis I had to use it to prepare the logistics of my next steps – it should not be underestimated the time and effort that goes into planning and arranging a multi-sited ethnography across different countries and regions within countries. The more profound reason, however, is that having two different phases for the data collection and the analysis is also compliant with certain schools of doing ethnography I feel methodologically closer to (see Hess, 2001). I will come back to this point at the end of the chapter.

countries. During the central stretch of my fieldwork I attended several events that were either relevant to my main case study or tangential meetings that were useful for building up interactional expertise and interacting with actors<sup>104</sup> – I managed sometimes to conduct face-to-face interviews at these ‘other’ meetings as well. I shall follow with a few details of events observed:

- *North Sea Regional Advisory Council (NSRAC) – Demersal Working Group Meeting (London, October 2006, with requested permission)*

This was the first event of the NSRAC<sup>105</sup> that I had the opportunity to observe. At this meeting ICES presented the TAC advice for the 2007 to stakeholders – namely different representatives of the fishing industry across the North Sea, some environmental NGOs and other NGOs representing the interests of the fishing communities. The discussions proved to be very useful for my own understanding of the stock assessment process and its shortcomings. With verbal informed consent I was allowed to record the meeting for the purposes of my doctoral studies and use the material under guarantee of anonymity – removing the possibility of distinguishing real names.

- *Joint NSRAC/NWWRAC<sup>106</sup> Symposium on Cod Recovery (Edinburgh, March 2007, open meeting)*

The cod fishery in the North Sea is central to any fisheries policy in the area. At the time of research stocks were endangered and the European Commission had established recovery measures. This event gathered a range of different constituencies – for instance, representatives of the fishing industry, fishing communities and environmental NGOs, policy-makers from different directorates at the European Commission and also from Norway, and fisheries biologists,

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<sup>104</sup> It has to be said that in all these meetings, whether they were central to the case study or tangential, it was very much the same actors that I was following around.

<sup>105</sup> I shall introduce this organisation in detail in the following chapter.

<sup>106</sup> NWWRAC stands for ‘North Western Waters Regional Advisory Council’.

economists, social scientists. This was a good opportunity to understand first-hand the reform of the Common Fisheries Policy and also its influence on the most iconic fishery of the North Sea – as well as on other North Sea fisheries intertwined with the cod fishery, such as the flatfish fishery.

- *UK Department for Environment, Food and Rural Affairs (DEFRA) – Biodiversity 2020 Strategy (Edinburgh, March 2007, by invitation)*

In 2007 DEFRA ran a consultation over their ‘Biodiversity 2020’ strategy, which was being produced at the time of research and aimed at sustainable fisheries in the UK. I attended a presentation of the main strategic lines to a group of British fisheries economists. Although the context of the discussion was only not directly related to the EU, it was a good opportunity to observe the fisheries economists in action and how they framed – and monetise – ideas around biodiversity and sustainability.

- *‘Developing a Socio-economic Data-frame for Fisheries Management Evaluation’ Project (Peterhead (UK), March 2007, by invitation)*

I was offered the opportunity to collaborate collecting socio-economic data for a research project run by some members of the NSRAC in a number of UK locations and funded by the DEFRA ‘Ad Hoc Science Programme’. The idea behind the project was to explore how to elicit and process data about the socio-economic impacts introduced by the European Commission’s policies. Together with other members of the project, I had the opportunity to enter in direct contact with the fishing industry in Peterhead. By and large, it was a valuable experience to learn about the problems with capturing social sciences’ data suitable for integrated impact assessment exercises.

- *Dutch Institute for Marine Resources and Ecosystem Studies (IMARES) – ‘F-Project’ (Stellendam (NL), March 2007, by invitation)*

This was a project commissioned by the Dutch Ministry of Agriculture, nature and Food Quality that aimed at facilitating cooperation between fisheries managers, biologists and actual fishermen in order to improve the quality and transparency of the advice for the management of the North Sea flatfish fishery. The project lasted five years and I attended one of the meetings between the scientists and the fishing industry. It was conducted in Dutch and I could not follow many of the details, but it was still worth observing because of the use of graphs and other self-explanatory visual information. It was a very good opportunity to get direct access to the real flatfish fishermen – i.e. beyond their spokespersons in the NSRAC.

- *Scientific, Technical and Economic Committee for Fisheries (STECF) – Long-term Management of Sole and Plaice: Assessment of Impact, Assessment of Methodology (Copenhagen, March 2007, with requested permission)*

This event – SGECA-SGRST-07-01 in its formal internal denomination at STECF – was central for the development of the main case study. At this meeting I could observe the fisheries biological and the economic modellers in action in the context of an integrated impact assessment exercise. The meeting was a follow up of the first workshop in September 2006 that I was not able to attend, as I mentioned above. In this respect, attending this second meeting also enabled me to visualise – and informally discuss – some of the relevant outcomes of the first workshop. Following informed consent I was able to tape record the meeting for the purposes of my doctoral studies and use the material under a verbal guarantee of anonymity promised – removing the possibility of distinguishing real names.

- *North Sea Regional Advisory Council – Demersal Working Group Meeting (Copenhagen, April 2007, with requested permission)*

This was the last event that I attended and another important one for the development of the main case study. By that point in time, the policy process that I had been following was nearly approaching an end and this was a good opportunity to observe how the actors settled several of the issues at stake. I received verbal informed consent to take notes of the meeting for the purposes of my doctoral studies under a

verbal guarantee of anonymity. I treated the notes in exactly the same way as the transcript of a recorded event and also removed the possibility of figuring out real names.

It is noteworthy that central for ethnography is the idea of demonstrating that one has been ‘there’. However, in a multi-sited ethnography the ‘being there’ and ‘doing research’ have to be treated in a different way. As Marcus (1995) points out, in this context the (mobile) analyst faces “the recalibration of affinities for, affiliations with, as well as alienations from, those with whom he or she interacts at different sites” (1995:113). According to Marcus, these circumstantial engagements on multiple locations generate a sense of doing more than ‘detached anthropological ethnography’; they provide in effect a substitute for the sense of ‘being there’. I certainly found that it was useful for me to engage sometimes as a genuine technical person – for instance, with the modellers – other times as a social scientist – for instance, with the fishing industry – and yet other times as a policy analyst – mostly with policy-makers.

### **3.4 *The analysis and the reflexive analyst***

Much of what I have commented upon so far has to do with ‘doing fieldwork’, with ‘being *in* the field’. Now I am going to reflect on the opposite effort, that is, moving out, allowing the analyst to emerge. Here I am not referring to the coding of the empirical material according to specific social categories such as values, imaginaries, expectations, interests and so on in a theory-driven approach, which I did not follow from the start. I mean instead the point at which the researcher feels that she has reached the end of the fieldwork, stops following the actors and steps out, taking distance from the field in order to digest and map out the empirical material<sup>107</sup>. This

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<sup>107</sup> Notably, Latour captures in his idiosyncratic style the challenging and difficult times of this process as follows:



is not how traditional ethnography is understood, but it is nonetheless ethnography, if only of a different kind because of its two distinctive phases, as signalled by Hess:

Failure to engage in the ‘stepping in’ and ‘stepping out’ process constitutes ‘going native’, which is usually rejected [...] the first concern is to understand how the world works from the point of view of one’s informants, thus to achieve competence in the culture. The distancing or strangeness that Latour and Woolgar [1986] wanted occurs with the stepping back process of social scientific analysis of one’s observations (2001:237).

‘Going native’ in this context takes place when the researcher becomes naturalised by those who have been interviewed or observed and takes up their accounts at face value, as Hess points out. Following the actors and trying to learn how they see the world can lead to these situations. The analyst’s views are inevitably shaped by the views of the informants, especially in dialogical settings such as face-to-face interviews (MacKenzie, 2005; see also England, 1994). The solution to minimise the naturalising effects of the field on the analyst proposed in ethnographies such as Latour and Woolgar (1986) is to postpone the analysis to a point in time where there is enough distance from the field. In my case, distancing from the field was largely helped by the course of my professional life beyond the PhD, which led me to interrupt my studies for a long period. Admittedly, this is somehow a radical

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How does one *make sense* of this mess as it piles up on our desks and fills countless disks with data? Sadly, it often remains to be written and is usually delayed. It rots there as advisors, sponsors, and clients are shouting at you and lovers, spouses, and kids are angry at you while you *rummage* about in this dark sludge of data to bring light to the world. And when you begin to write in earnest, finally pleased with yourself, you have to sacrifice vast amounts of data that cannot fit in the small number of pages allotted to you. How frustrating this whole business of studying is (2005:123, my emphasis).

In hindsight, I find that this is probably the price to pay for learning ‘*tabula rasa*’. It demands collecting lots of evidence and takes a lot of time – not only to collect the data but also to analyse it. I personally experienced some of the difficulties that Latour describes, struggling for a long time to get a good sense of control of where the analysis of all the evidence collected in the field was leading – even after taking considerable distance.

stepping out but it has given me with a much-needed analytical distance at the time of writing up.

By and large, not only is the analyst changed to a greater or lesser degree by doing fieldwork, the other way round should also be taken into consideration. Part of the role of the analyst is to reflect on her influence over the field (Toren, 1996). Following England (1994), it is interesting to focus on how the biography of the researcher influences the fieldwork. Fieldwork is personal and thus the personal characteristics and biography of the researcher matters. That I am, for instance, a white, male Spanish engineer doing a PhD in Science and Technology Studies at the University of Edinburgh and living across a few coastal countries may have led more easily to particular conversations and insights – for example, male-gendered technical conversations on modelling approaches. And perhaps if I had been a senior academic, informants would have said things differently sometimes. It follows that fieldwork is a dialogical process structured by the researcher *and* the informants in response to the researcher's presence as well as her biography; that is, a process exposed in general to power relationships (England, 1994).

To end the chapter with a concrete and telling example of the effects that my presence brought to the face-to-face interviews and conversations with informants at different meetings, I can highlight the desire of a number of them to engage with me as a STS researcher in a reflexive manner, sometimes even pointing at some classic STS literature. While some of the informants from the Commission or indeed the fishing industry had a publication record or a background in social and political sciences – which is remarkably enough – this was even more outstanding in the case some of the biologists and economists<sup>108</sup>. It looked as if the fisheries biologists and

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<sup>108</sup> Funtowicz and Ravetz's notion of 'post-normal science' (1993) was fairly well known by several of them. In both case studies I also came across at least one scientist that had read with interest 'Science in Action' by Latour (1987). As very special cases, one of the fisheries scientists and one of

economists had developed a distinctive way to talk to social scientists over the course of the long tradition of social research on fisheries, which has often found useful some of the concepts and ideas developed in STS. By presenting myself as doing social sciences research on science and policy I would be opening the door for the interviews and conversations to reach some degree of abstraction – for instance, towards the end of the interview some informants would want to take conversation to ‘broad issues’ such as, *inter alia*, the relationship between science and politics or their challenging job as scientists working very close with policy. While I am clearly not arguing that the informants turned into reflexive actors in the course of our conversations, I believe that my presence and research interests invited reflexivity to emerge<sup>109</sup> and, in so doing, influenced the general tone of the conversations and some of the specific responses that I got from my informants.

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the fishing industry representatives that I interviewed had trained in STS – the latter had even attended the ‘Actor Network and After’ Workshop at Keele University in July 1997 (see Law and Hassard, 1999). All in all, having an STS PhD student across the table was an opportunity for them to discuss some of their STS background knowledge as well as their views on STS.

<sup>109</sup> On this point see also Knuuttila (2002).

## 4 The reframing of the EU fisheries policy arena around 2002

### 4.1 Introduction

In the heavily regulated waters of the EU<sup>110</sup>, the turn of the 21<sup>st</sup> century was received with an attempt to overcome the problems that had been affecting EU fisheries management for a while. Throughout the chapter I will provide an account of the crisis picture of the EU fisheries regulatory arena that led to the changes to the regulatory framework in 2002. Some of those changes were masterminded for a new Common Fisheries Policy (CFP); some were conceived outside the world of EU fisheries management and had a very significant influence in the constitution of the new regulatory arena at this particular time. Therefore, the idea in this chapter is to provide a thorough account not only of the problems that were affecting the management of EU fisheries around the year of 2002, but also of the solutions that were suggested since it is these solutions, and in particular how they worked, that I will discuss in the following chapters through a case study of the North Sea flatfish fishery.

Notably, the combination of internal and external changes to the regulatory framework in 2002 provided a unique opportunity for the study of EU fisheries management and its associated technologies<sup>111</sup>, while at the same time it became a bit of challenge to ‘navigate these unsettled waters’. During my fieldwork, I often found myself trying to make sense of the extent of all these changes, to the point that I ended up collecting meaningful empirical material that went beyond the actual case study centred on the North Sea flatfish fishery. It is for this reason that this chapter

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<sup>110</sup> As some social scientists have put it, “perhaps the most top-down fisheries management system on the planet” (Degnbol and Wilson, 2008:189).

<sup>111</sup> In anticipation of the later discussion, and bearing in mind that Callon’s (1998) ‘hot’ and ‘cold’ situations should not be read as a binary account of the policy world, it is helpful to imagine the situation of EU fisheries management under the CFP and with its TAC Machine as getting rather ‘heated’ around 2002 and the regulatory reforms as a means to ‘cool’ the TAC Machine down.

has a clear empirical flavour, even if at various parts I still recur to the relevant literature – for instance to offer some of the prehistory of EU fisheries management.

The structure of the chapter is as follows. In §4.2 I will draw on the scholarly literature to describe how fisheries management and fisheries science were organised in the North Atlantic, particularly towards the end of the 20<sup>th</sup> century and focusing mainly on the EU. Notably, the CFP as first illuminated in 1983 became critically problematised by different actors and, in §4.3, I will draw on empirical material and introduce the regulatory reforms and communications put forward in 2002 by the Directorate-General for Fisheries and Maritime Affairs of the European Commission – hereafter DG-FISH<sup>112</sup> or simply the Commission – in response to the legitimacy crisis. In §4.4, drawing on the literature I will describe another major steering, this time in the whole European Commission, with the establishment of the ‘Better Regulation’ strategy, which for DG-FISH meant eventually the addition to the reformation of the CFP of mandatory *ex-ante* ‘bio-economic’ impact assessments. Then, based on empirical insights, I will consider how the need to conduct integrated impact assessments was received in DG-FISH. In §4.5 I will offer an empirical account of how certain issues remained after the reform in 2002, especially when it came to the problematisation of stock assessments by the fishing industry. Finally, in §4.6 I will offer some conclusions to a chapter that, overall, will serve as a backdrop to the main case study that I will introduce straight afterwards.

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<sup>112</sup> At the time of research this is how the ‘Directorate-General for Fisheries and Maritime Affairs’ was commonly (and informally) referred to. As of 1<sup>st</sup> of April of 2008, the new name of the directorate is ‘Directorate-General for Maritime Affairs and Fisheries’, and is commonly shortened to DG-MARE. I will continue to use DG-FISH throughout the chapters of this thesis to be consistent with the time of research. Unless stated otherwise, I will be referring to the unit responsible for conservation policy within DG-FISH.

## **4.2 The regulatory framework for EU fisheries management before 2002**

In this section I will describe how fisheries management and fisheries science were organised in the European Union before it underwent its first large reform in 2002<sup>113</sup>. I will address this in three steps. First, I will inform a prehistory of fisheries management and fisheries science centred on the North Atlantic. Second, I will describe the creation of the Common Fisheries Policy in 1983, which only underwent its first serious reform in 2002. Third, I will address how the scientific advice was institutionalised under the CFP introduced in 1983.

### **4.2.1 The prehistory of fisheries management and fisheries science coming into the European Economic Community**

Let us focus first on some of the prehistory behind fisheries management and its close interaction with fisheries science, in an account centred in the EU but that necessarily goes beyond the geographical boundaries of the EU and remits to the North Atlantic fisheries.

Scientists were already informing the debate on the need or not for fisheries management as early as the end of the 19<sup>th</sup> century. In 1883 the renowned English biologist Thomas Huxley boldly claimed that the most important world fisheries were inexhaustible relative to the pre-industrial fishing technologies of the time<sup>114</sup> (Pálsson, 1998; Bavington, 2009). The dominant scientific and expert consensus up to the end of the 19<sup>th</sup> century echoed Huxley's belief that one should not worry too

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<sup>113</sup> The CFP touches on four pillars: resource conservation policy, structural policy, common organisation of the market, and a policy of agreements with third countries allowing access to fishing areas. In the 1992 interim review of the CFP, there were no fundamental changes but adjustments between the declining resources in the sea, the structural size of the fleets and the market mechanisms (Symes, 1997). The review in 1992 indeed stipulated that the Fisheries Ministers had to decide by 2002 on any reforms to be made to the CFP after hearing a proposal from the European Commission (Lequesne, 2004).

<sup>114</sup> The fishing vessels only started to benefit from industrialisation around 1890 with the introduction of steam trawlers in England (Bolster, 2006).

much about managing the fishing activity to prevent the exhaustion of the stocks. The impact of human activity compared to natural fluctuations was insignificant. However, the rapid industrialisation of the fleets that followed suit raised doubts within the fishing communities in Europe and North America and turned tighter fisheries management and new regulations into a pressing need, particularly at the start of the second half of the 20<sup>th</sup> century (Bavington, 2009).

Notably, the exploitation of the seas at large had been a set issue in international law for centuries before that. It is worthwhile to go all the way back to 1633, when the Dutch jurist Hugo Grotius codified an old Roman principle known as ‘Mare Liberum’ – that is, Freedom of the Seas – that established unrestricted use of non-territorial seas for military and commercial navigation and fishing in times of peace (Bavington, 2009; Holm, 1996; Van Ittersum, 2007). The principle would allow the Dutch to break up various trade monopolies of the most powerful naval nations at the time and take part in the lucrative trade with the East Indies. Since the days of Grotius and until well into the second half of the 20<sup>th</sup> century, the fish stocks more than three nautical miles offshore<sup>115</sup> were considered open access and common property resources (Higginson, 1993). Notably, the beginning of the end for Mare Liberum came about in 1946, when awareness of the deployment of fish resources became evident at the ‘London International Over-Fishing Conference’. This event was followed by the publication in 1948 of the FAO State of Food and Agriculture report, illuminating the problem of fleet capacity excess (Garcia, 1992). Yet the

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<sup>115</sup> The three-mile limit of a country’s exclusive territorial waters is believed to correspond with the reach of cannons fired from land in the 18<sup>th</sup> century, although there are other interpretations (Anand, 1982). Mare Liberum found fierce opposition from England and the other dominant trading states, which fought back with the opposite principle of restricted seas – codified by the English jurist John Selden in his work ‘Mare Clausum’ (1635). Cutting a long story short, the disputes over maritime sovereignty between the coastal nations in Europe only reached some settlement at the beginning of the 18<sup>th</sup> century with the ‘cannon shot rule’. The solution, established by the Dutch jurist Cornelius Van Bynkershoek in his ‘De Domino Maris’ (1702), was to restrict national rights to the belt of water that the cannon range could effectively protect. The rule became widely adopted by the coastal nations mark their territory – meanwhile in the rest of the maritime domain Mare Liberum applied (Higginson, 1993).

freedom of the seas continued to be in place for a couple of decades more, during which pressure for a new oceans regime built up (Johnsen et al., 2009).

Since the early 1950s, industrial fishing grew exponentially<sup>116</sup>, thanks in part to the freezer trawlers and sonar technologies brought by WWII (Pauly et al., 2002). By the end of the 1950s the three nautical mile limit and the freedom of the seas were under high political pressure in the international policy arena. The United Nations had held two conferences on the Law of the Sea (UNCLOS) in 1958 and 1960. Yet, when it came to the extent of territorial waters, these two conferences – sometimes referred to as UNCLOS I and UNCLOS II – ended with no agreement (Higginson, 1993). In Northern Europe, the question of sovereignty over cod stocks led to what were famously known as the ‘cod wars’. These were three different episodes of confrontation between Icelandic and British governments and fleets – fishing and military – that took place in 1958, 1972 and 1976 after three unilateral extensions of maritime sovereignty by the Icelandic governments, the last one reaching 200 nautical miles (Ingimundarson, 2003). In 1977, the European Economic Community (EEC) followed Iceland – plus already the United States, Norway and Canada – and claimed the 200 nautical miles off the coastline as a ‘common resource’, which became internationally accepted as the limit for exclusive jurisdiction (Rozwadowski, 2002). It was the start of the closing of the commons. In 1982, a third UN Convention on the Law of the Sea was organised (UNCLOS III) and a formal agreement was reached. UNCLOS III set the maximum width of the territorial sea (inshore area) from 3 to 12 nautical miles and established the coastal states’ rights of management over an adjacent Exclusive Economic Zone reaching out to 200 miles from the coast (Higginson, 1993). UNCLOS III, which only entered formally into force in 1994 because it needed a minimum number of signatory

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<sup>116</sup> The reported global marine registered landings moved from about 18 million tons in 1948 to about 87 million tons in 1996 thanks to the industrial organisation and expansion of the fleet and the improvements in fishing technology (Caddy and Cochrane, 2001).



countries to ratify it, has ever since been regarded as the ‘mother law’ of fisheries management in the international arena (Cochrane and Doulman, 2005).

The long process towards a new oceans regime and the introduction of EEZs fisheries management was paralleled with the development of scientific expertise over the fish stocks; the promise was that the seas could be steered to produce socio-economic benefits by means of instruments for the distribution of fishing rights. In the 1950s, the dynamics of the fish populations were brought down to a number of key factors. In 1957 Beverton and Holt (1993[1957]) published what the fisheries biologists of the time working under the remit of the ‘International Council for the Exploration of the Seas’<sup>117</sup> considered as the ‘the Bible’ of fish population dynamics (Rozwadowski, 2002). The central feature was a fishery dynamics calculation model by which biologists could produce estimates of the average yield of a fishery under various conditions. Notably, it was an analytical model, which did not demand large data collection campaigns or even computation. All it required was three parameters; stock recruitment, growth and mortality. Drawing on the model, by the early 1960s the ICES scientists felt they were capable of estimating accurately the production of a fishery and suggest measures for policy on it.

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<sup>117</sup> ICES, a membership organisation of 20 countries, is a central actor in the institutionalisation of fisheries science in the North Atlantic and deserves a multifaceted characterisation, such as the following:

ICES is a lot of things. It is an intergovernmental scientific organisation which is charged with producing scientific advice to be used in official decision-making. It is a primary professional organisation for marine and fisheries scientists in Europe and an important secondary organisation for North American scientists. It is a building in Copenhagen where a professional staff processes data, edits kilograms of scientific advice, and constantly hosts meetings on many subjects. It is also a loose network where approximately 1600 scientists work together to address scientific questions, some of which they are eager to examine and some of which they wish had never been asked (Wilson, 2009:94).

For a thorough historical account of ICES see Rozwadowski (2002).

Yet the ‘mystery of the herring collapses’, that is, the ‘unbelievable’ decline of the North Sea and the Atlantic-Scandinavian herring stocks in the mid-1960s exposed a problem with the Beverton and Holt approach (Rozwadowski, 2002). The dominant belief across ICES so far had been that fish stock recruitment success was independent of the spawning stock’s size; the problem of overfishing only affected the spawning stock size because too many little fish were captured. In their ‘detective work’ to make sense of the herring collapses, ICES scientists began to question whether the combination of heavy fishing and poor year-classes could reduce the spawning stock to very low levels, resulting in more poor year-classes even if the environmental conditions for recruitment were good. In 1965 they came up with a new tool after the invention of Virtual Population Analysis by J.A. Gulland, a statistician by training. This new approach, which required the support of a calculation device, allowed them to specify the original stock size in retrospect by looking at how many fish from each year-class had been taken out of the sea for the lifespan of the year-class (Rozwadowski, 2002). Once you know the stock size and how many fish have been taken out of the stock every year you can consider setting quotas and minimum sizes to control the catching activity<sup>118</sup>.

VPA-based TACs has been the dominant approach to the distribution of fishing rights in the data-rich environments of the developed world post-1977 (Holm and Nielsen, 2004). Yet it is worth noticing that, before the EEZs emerged, there were really two options for the distribution of fishing rights in the international policy arena, governed at the time by the ‘International Commission for the Northwest Atlantic Fisheries’<sup>119</sup> (ICNAF). Each option carried a particular ordering of the relationship between science and management (Holm and Nielsen, 2004). On the one

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<sup>118</sup> Notably, the interest of the VPA approach is how it served hand in glove the production of the TACs for individual stocks in each of the fishing grounds, to the point that TACs were naturalised as the obvious outcomes of VPA model runs (Holm and Nielsen, 2004).

<sup>119</sup> The ICNAF was among the first regional fisheries management bodies to be established in the world and played a leading role in the assessment and management of fish stocks outside of national jurisdictions before the arrival of EEZs. It ceased its operations in 1979 (Anderson, 1998).

hand, there was the option supported by the US of capping the fishing effort exercised by the fleets at sea, the capacity to fish in other words. Notably, this option was less demanding on science than the other alternative (below) but difficult to implement and manage in practice. The problem this solution faced was that fishing effort is a moving target for any scientist trying to pin it down in some kind of measurable and universal units, thus making it politically sensitive. In other words, this option did not offer a neat division of labour between fisheries science and policy-makers – standardising fishing effort across fleets and countries raised political concerns over its unfeasibility. On the other side of the Atlantic, the Soviet Union strongly supported the option of setting quotas on catches or landings, which one could measure in a far less problematic unit of tons of fish. At least in principle, this alternative favoured this division of labour, with scientists assessing annually the fish stocks and advising on TACs and policy-makers approving or adjusting these limits to catches and agreeing on the country shares – in tons. Therefore, the TAC option was meant to simplify the political negotiations – though only at the cost of huge scientific work for data collection and assessment of fish numbers. This is in short how single stock TACs won over fishing effort limitations in the late 1960s for the main northwest Atlantic fisheries<sup>120</sup>. As follows, when the EEZs arrived in 1977 most countries had been already recurring to the TACs produced by ICES (Holm and Nielsen, 2004).

I shall now move to detail the regulatory framework that during the period 1983-2002 informed fisheries management in the European Economic Community – later the European Union. And since the EU has always looked to science for the legitimisation of its fisheries policy, enacting the rationalistic approach to decision-

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<sup>120</sup> Yet, as soon as in 1973, ICNAF realised that TACs for the commercial stocks alone were not effective in reducing the fishing activity – fishermen moved to those species still unregulated (Holm and Nielsen, 2004). Ever since how to reinforce the single stock TACs has been an issue at stake and fishing effort control reconsidered from time to time in one way or another.

making described above, I will also address the scientific advice that was instrumental for the Common Fisheries Policy.

#### **4.2.2 How EU fisheries management was institutionalised before 2002**

European Economic Community regulation for the creation of specific measures for fisheries was adopted by the European Agricultural Ministers in 1970 (Lequesne, 2004). The aim was to establish a common market for fishery products along the same template as for fruit and vegetables, accompanied by structural funds from the Community budget to improve production. As with the single market, the idea was also to make the territorial waters of all member states a common resource open to all fishermen from those countries – Belgium, France, Germany, Italy, Luxembourg and the Netherlands. The idea had started to be discussed already in 1966 and the applications for membership from Denmark, Ireland, Norway and the UK in 1969 speeded up the process. These four countries were fishing powers and each had their own fisheries policies and, in dealing with the negotiations for their accession, the Community felt it should have a fisheries policy of its own (Lequesne, 2004).

The fact that these four countries were wealthier in marine fish resources compared to the founders of the EEC, coupled with the idea of a common and free access fishing resource across the whole EEC, created tensions over the course of the negotiations for accession, especially under the expectation of soon establishing rather large EEZs. For instance, in the early 1970s French fishermen made substantial catches in the high seas that were bound to become part of the British EEZ and, to a lesser extent, Norway's soon to become EEZ. Meanwhile, the governments of the four applicant countries demanded exceptions to the principle of

free access<sup>121</sup>. By and large, the Common Fisheries Policy was already controversial in those early days (Holm and Nielsen, 2004).

In 1977, EEC declared the new exclusive economic zone to be administered directly from the European Commission, which would have the power to conclude fisheries agreements with third countries (Lequesne, 2004). The largest of its kind, the problem was namely how to organise access for all EEC members – and potential countries to join – to the now vast fishing resources under Brussels control. How to systematise the distribution of fishing rights triggered a political process that illuminated the first unequivocal Common Fisheries Policy in 1983, after six years of negotiations (Farnell and Elles, 1984; Holden, 1994; Holm and Nielsen, 2004; Nielsen and Holm, 2007; Lequesne, 2004). The pivotal aspect of the negotiation was the establishment of the principle of ‘relative stability’. The agreement was that each Member State’s annual relative share of the different fish stocks should remain fixed over time (Holm and Nielsen, 2004). The principle should observe additional rights that had been assigned to certain Member States as a compensation for the loss of historical fishing rights in distant waters (high seas) after the arrival of EEZs (Farnell and Elles, 1984; Lequesne, 2004; Symes and Crean, 1995). By and large, implementation was envisaged through annual TACs for each single species stock that could then be divided into country quotas<sup>122</sup>. Therefore, all in all, relative stability and TACs illuminated in 1983 the first fully-fledged Common Fisheries Policy, thus combining concessions to the fishermen who had lost out with the arrival of EEZs.

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<sup>121</sup> Indeed, while Denmark, Ireland and the United Kingdom joined the EEC in the end, Norway allegedly refused to join because of the transfer of sovereignty over their fish stocks to Brussels – ‘fish stealing’ as it was perceived (Holm and Nielsen, 2004), a theme exploited by the political parties opposed to joining the EEC ahead of the negative vote in the referendum of 1972 (Lequesne, 2004).

<sup>122</sup> There are no annual TACs and quotas set by the EU institutions for Mediterranean fisheries. Given that the Mediterranean was not touched by the new EEZs regime in 1977, the EC fisheries ministers decided it was exempt for the TAC regulations (Lequesne, 2004). In the case of Bluefin tuna fished in those waters, there are TACs limiting catches in both the Mediterranean and the Atlantic but these are set by the ‘International Commission for the Conservation of Atlantic Tunas’ (ICCAT). The quotas are distributed amongst ICCAT members, including the European Union.

Created in 1977 out of the fisheries department that was in the Directorate General for Agriculture, DG-FISH – now DG-MARE – has been the body responsible for the implementation of the CFP. During the period 1983-2002 – and immediately after (see Gray and Hatchard, 2003) – they applied a hierarchical and mechanistic (Burns and Stalker, 1994[1961]) organisational style and, notably, became the strict guardians of expert rationality in the face of the member states subjected to the pressure of their fishermen. DG-FISH officials – and particularly those in the directorate in charge of resource conservation policy – shared with the ICES community – to which some of them originally belonged – the same initial ideas about what constituted ‘good’ fisheries policy, namely to avoid the tragedy of the commons. In this sense, the Fisheries Commissioners – rarely with prior experience with fisheries problems – have always given DG-FISH a relatively large degree of autonomy in the development of the CFP (Lequesne, 2004).

However, it is worth noting that annual quotas during the 1983-2002 period were ultimately and exclusively passed by the Council of the European Union<sup>123</sup> – the Council of Ministers – every December after hearing the Commission’s proposals for quotas and holding face-to-face negotiations with Norway. Furthermore, it is also important to understand that Member States have been able to choose how to distribute across their national fleets the quotas allocated to them in Brussels. In this sense, there are significant differences on how to implement the quota regulations, subject to each national context (Lequesne, 2004). For instance, some Member States have implemented a market mechanism with the introduction of an ITQ system for their fishermen – e.g. the Netherlands. By and large, Lequesne points out, implementation and monitoring not only influence fishermen’s compliance but also illustrate overall particular relationships between the national state and the fishermen in the management of public goods.

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<sup>123</sup> The European Parliament had only a simple consulting role at the time.

Having presented the CFP as a frame for allocating fishing rights in accordance with the principle of relative stability, on the other hand it has failed the conservation of the stocks (Holden, 1994; Holm and Nielsen, 2004). While there are empirical studies showing that the deviation from the ICES advice in the setting of the quotas had not been particularly significant over the 1983-2002 period (Patterson and Résimont, 2007), research shows as well that, up until 2003 at least, there was only evidence of one stock where the administrative technologies of the CFP had actually influenced positively its development (Sparholt et al., 2007). Let us now examine in more detail the scientific support demanded in this regulatory framework.

#### **4.2.3 How EU fisheries scientific advice was institutionalised before 2002**

In 1977, with a large number of stocks under its jurisdiction, the EEC did not have the scientific infrastructure of its own to produce stock assessments so it decided to stick to ICES for TAC advice, and more precisely to its policy-advice-spearhead, the ICES ‘Advisory Committee for Fisheries Management’ (ACFM), created in the same year. This was not only a matter of convenience, countries like Norway with which the EEC shared some stocks insisted on seeking advice from ICES. However, by the early 1980s Brussels officials started to consider having their own machinery. In the eyes of the EEC members ICES had become strictly conservationist and some countries were simply not enforcing the quotas. EEC officials began to revise the TACs taking into account other considerations rather than conservation of the stocks. In this sense, they developed an advisory structure within the remit of the EEC, just like every individual country represented at ICES had – the important difference being that this did not grant the EEC fisheries experts access to the core of the ICES stock assessment process, as reflected in the first Memorandum of Understanding between ICES and the EEC under the Common Fisheries Policy (Rozwadowski, 2002).

ICES has in the European Commission its most important client (Wilson, 2009) and regular ‘Memoranda of Understanding’ has been signed between both parties as formal agreements for the provision of advice based on single species stock assessments (Delaney and Hastie, 2007). This is a vast amount of work. To produce the annual TACs, scientists had to carry out single-species stock assessments every year and the single-stock TAC instrument had to be applied for all major commercial stocks for it to be of any effect<sup>124</sup>.

Holm and Nielsen (2004; see also Nielsen, 2008; Schwach et al., 2007) used their notion of the ‘TAC Machine’ to describe the effortful annual cycle of stock assessment and advice on catch options by ICES, the proposed TAC by the European Commission to the Council of Ministers, and the final decisions by the Council. This data intensive annual routine of assessing the size of single species fish stocks based on the VPA approach<sup>125</sup> can be depicted as follows for the 1983-2002 phase (ICES, 2007a). First, ICES scientists monitored in the sea the adult fishes with reproductive capacity in each stock – known technically as the spawning stock biomass. To come up with estimations of this biomass for each of the commercial fish stocks ICES scientists collected data from the fishing fleets registered at the landing ports but they also ran their own surveys. These research surveys not only allowed the scientists to validate and fine tune the data that came from the commercial fleets but also to estimate the stock recruitment – i.e. the production of offspring. ICES scientists in different working groups according to fish species and fishing grounds then estimated the numbers of adults for the upcoming years, which they did by

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<sup>124</sup> Some 120 stocks of fish and shellfish in total (Lequesne, 2004).

<sup>125</sup> As Holm and Nielsen (2004) point out, the VPA instrument served hand in glove the stock assessment process, where it became entrenched. This was not because of the inner properties of the VPA approach. Technically speaking, the VPA could have served for other management approaches such as effort quotas. However, Holm and Nielsen’s argument follows, the TAC Machine aimed at a clear division of labour between science and policy and this was largely made possible by using the VPA as a method for counting the fish in tons. The resilience of the VPA in the stock assessment process was ultimately enabled by the TAC Machine success as a method for the distribution of fishing rights that allowed the implementation of the relative stability principle.



considering the amount of juveniles that would mature relative to the estimated fishing and natural mortalities.

Around October or November each year ICES – the ACFM body to be more precise – reviewed and approved the TACs for each stock and fishing ground suggested by the different working groups and briefed the final TAC advice to the European Commission. DG-FISH then sent the ICES ACFM advice to the ‘Scientific, Technical and Economic Committee on Fisheries’<sup>126</sup> (STECF), where a group of mostly fisheries economists took up the TACs calculated at ICES and conducted a cost-benefit analysis of what these TACs would imply for the fishing fleets – so that not only biological considerations were part of the process. STECF normally ratified the advice from ICES without asking for modifications (Lequesne, 2004). After receiving economic advice from STECF, DG-FISH then had to, first, agree with Norway on the shared stocks, and second, decide what to propose to the Council of Fisheries Ministers in terms of quotas, ahead of the annual political negotiations in December. This was the final stage in the decision-making process where the quotas for the following year in the EU waters were set. Notably, DG-FISH officials often passed on the ‘pro-conservation’ advice from ICES knowing that the national ministers would review the proposals to serve the economic interests of their respective fleets<sup>127</sup>. By and large, at the Council of Ministers in December the

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<sup>126</sup> The expert rationality at DG-FISH was reinforced in 1979 with the creation of STECF, the own advisory body of DG-FISH, which it funds directly. The idea was to enable the fisheries biologists to comment on the Commission’s TAC proposals made following ICES advice in principle. However, in 1993 the Commission called in economists to explore issues from a social sciences perspective and contribute to specific problems such as fishing industry excess capacity or competition. Inspired by the success of ICES for the biologists, the fisheries economists in Europe created in 1998 the European Association of Fisheries Economists (EAFE) to strengthen their role within the production of advice to DG-FISH. Yet their attempts to project themselves over the biologists did not succeed (Lequesne, 2004).

<sup>127</sup> As John Gummer, formerly UK Minister of Agriculture, Fisheries and Food (1989-93) and Secretary of State for the Environment (1993-97), put it: “If you are a fisheries minister you sit around the table arguing about fishermen—not about fish. You’re there to represent your fishermen. You’re there to ensure that if there are ten fish you get your share and if possible a bit more. The arguments aren’t about conservation, unless of course you are arguing about another country” (O’Leary et al., 2011, citing an article in ‘Fishing News’, 18 December 1998).

question was how to keep social peace in the national contexts, even if the fishermen were relatively low in numbers in most countries (Lequesne, 2004).

It is noteworthy that the scientific advice produced by the TAC Machine was not particularly well tuned against biases, at least until the turn of the 21<sup>st</sup> century<sup>128</sup>. Following the FAO Code of Conduct on Responsible Fisheries agreed in 1995, ICES introduced the precautionary principle that had already taken currency in environmental science and management. Applied to fisheries, the idea was to avoid the risk of collapse in the fish stocks by keeping the spawning stocks and the fishing mortality at a precautionary distance from the levels scientifically considered risky for each stock. ACFM established an operational definition in 1998<sup>129</sup> (ICES, 1998) and, thereafter, the annual ACFM advice on each single stock was given on the basis of this definition. Accordingly, DG-FISH linked the application of the precautionary approach to the setting of TACs (European Commission, 2000). However, despite the introduction of the precautionary principle the general rationale of fisheries management continued unchanged. Fisheries management simply had developed into another example of risk management (see Degnbol, 2004) also in the EU<sup>130</sup>.

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<sup>128</sup> Notably, for all stock assessments up to 2002 the TAC Machine overestimated the stocks' size, incoming recruitment and growth and the annual forecasts were overly optimistic (see Reeves and Pastoors, 2007).

<sup>129</sup> The precautionary principle was operationalised in ICES by means of a grid of reference points for both spawning stock size and fishing mortality – not only low stock size triggers an alarm but also a high fishing mortality, since it precludes a low stock size in the near future. For each fish stock, there were limit reference points for fishing mortality and biomass (*Flim* and *Blim*), which marked the high risk of collapse, and precautionary reference points (*Fpa* and *Bpa*), which signalled a lower risk of stock collapse (ICES 1998). In other words, doing precaution in fisheries translated in allowing more room for error in the diagnosis of the risk of collapse (Degnbol, 2004). In principle, given a single stock if the spawning stock biomass was above the precautionary level and looks to remain like that then the TAC was set to keep the status quo. If not, the TAC was established so that the stock returns to precautionary levels within one year. At the same time, it could be that the spawning stock biomass levels were fine but the intensity of fishing mortality could cause problems further down the line. In this case, the TAC should also be reduced to restore the precautionary levels in terms of fishing mortality within one year.

<sup>130</sup> Notably, sometimes error margins for stock assessments went up to 50%! (Daw and Gray, 2005).

A strong matter of concern faced by the rationalistic approach to fisheries management in the EU was the systematic alienation of the fishing communities from the centres of decision-making and production of scientific advice. The CFP introduced a linear division of labour between scientists providing sound advice and policy-makers at the European Commission deciding on this advice before putting it forward to the Council of Ministers, and the fishing industry was simply left out of the governance approach. In response, the fishing industry often problematised the stock assessment and the underlying modelling process as a source of legitimacy, pointing at the same time to the overall deficit of legitimacy in an approach that excluded the fishing industry from the governance of EU fisheries. Degnbol summarised the issues at stake in the following terms:

The main concept is still that it is possible to establish objective goals and [scientifically sound] means to achieve those objectives can be deployed. There has been increasing awareness of the need to include legitimacy as an important objective in its own right, but still remains to be demonstrated whether it is possible to introduce legitimacy [given this dominant rationale] [...] [These models] are received with considerable scepticism in fishing communities [...] This cannot just be explained by lack of understanding of the technical details of the models [...] Important components could be the fact that the models are operating on a very different [time and spatial] scale from the fisheries, making it difficult for fishermen to recognise the model results from their everyday experience, and that the conceptual basis of the models has moved far away from any intuitive understanding of how marine ecosystems work. Underlying the differences in concepts and scales are the differences in interest between fishermen and scientists – the *raison d'être* for fisheries science has been and still is that the management bureaucracy needs technical tools to implement its rationality and it has been the interest of scientists to justify their work by providing these tools (1998:8).

In the next section I will describe how the European Commission responded to the crisis picture described in this section by introducing a reform of the CFP in 2002. In so doing, rather than relying (exclusively) on the scholarly literature in order to illustrate the changes introduced, I will begin to offer some empirical material from my fieldwork.

### **4.3 The reform of the Common Fisheries Policy at DG-FISH in 2002**

The framework set by the original CFP failed the conservation of fish stocks in EU waters dramatically, as revealed by a 50% decline in demersal fisheries landings over its nearly 20 years lifetime (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). In 2001 the Commission listed the main shortcomings of the CFP in a ‘green paper’ anticipating the reform<sup>131</sup>, making reference to the weaknesses in the short-term biological advice, the discontent of the stakeholders – particularly the fishermen – with the lack of long-term management strategies, the excessive attention to single species fish stocks instead of the fisheries as a whole with their multiple and intertwined stocks, lack of environmental considerations – e.g. ecosystem views – and the chronic overcapacity of the fleet and poor enforcement of the management measures (European Commission, 2001c). This green paper opened up a consultation process with stakeholders whereby DG-FISH received many responses on both why the CFP was perceived as failing and how it could be improved. It was the measure of the crisis picture affecting the CFP.

In 2002 the Council of Ministers adopted a number of decisions to overhaul the CFP (Council of the European Union, 2002). The pursuit of regulatory reforms introduced four important policy innovations (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). First, stakeholders’ involvement in the policy processes with the establishment of ‘Regional Advisory Councils’ (RACs), whereby DG-FISH wanted to improve the dialogue with stakeholders. Second, a shift from the fish stock to the mixed fishery as the unit of action – i.e. taking into account all the fish stocks caught in a given fishery. Third, the introduction of long-term management plans, with quantitative end targets and multi-annual ‘harvest control rules’ (HCRs) that established both catch limits and constraints to the time and resources employed to fish. Annual TACs should not

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<sup>131</sup> To recall, the reform of the CFP in 2002 was mandated by EU legislation.

disappear<sup>132</sup> but the idea was to undergo a fundamental change in the policy process from the annual politically led decisions to decisions based on the annual implementation of a long-term strategy with long-term objectives. The idea was to tie up the hands of the Council of Fisheries Ministers in December, so that the TACs and quotas decisions that they took were not just *ad hoc* decisions out of political negotiations in the early hours of the day but decisions that came from the application of a sound long-term management strategy. Finally, the fourth important issue was the willingness to adopt environmental objectives as the end targets for the long-term management plans. I will now unfold each of these points.

#### **4.3.1 Regional Advisory Councils**

Establishing the Regional Advisory Councils was a crucial element of the reform of the CFP in 2002. Finally adopted by the Council of Ministers in 2004, the RACs were created to involve geographically bounded stakeholders directly in the provision of policy salient advice to DG-FISH – notwithstanding the previous existence of another stakeholder platform, the much broader ‘Advisory Committee on Fisheries and Aquaculture’<sup>133</sup> (ACFA). Overall, they were the institutional response to the discontent of the industry with what they saw as lack of legitimacy in the decision-making process and poor communication. Accordingly, the internal structure of the RACs reflected the weight of the fishing sector, with two thirds of the seats in the executive committee. The other third of the seats went to other interest groups such as environmental NGOs, fishing communities’ networks, recreational fishermen and consumer organisations, all wishing to enter a dialogue

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<sup>132</sup> As Nielsen and Holm point out, that Brussels tried to fix the shortcomings of the TAC Machine “by way of tinkering” (2007:677); they observe that “the TAC Machine to some extent functions as the platform from which these reform discourses are launched and played off against each other. The TAC machine, in other words, would work if it was tweaked in the right (disciplinary) direction” (2007:678).

<sup>133</sup> Prior to the emergence of the RACs, the voice of the stakeholders was channelled in principle through the Advisory Committee on Fisheries and Aquaculture. After the reform of the CFP in 2002 ACFA has continued to exist but differs from the Regional Advisory Councils. ACFA has the right to express its views on any subject relating to fisheries in general, such as health issues or questions regarding fishing and aquaculture markets and products (European Commission, 2005c).

with the professionals and the policy institutions responsible for fisheries management, and to defend their points of view<sup>134</sup>. Furthermore, there was clearly a need to improve cooperation between fisheries professionals and scientists responsible for advice on the state of fish stocks. In this sense, the RACs represented in principle a permanent forum for dialogue and co-operation between these two sets of actors.

The reform of the CFP envisaged seven different RACs in order to ensure that the actors who shared the same kind of problems were together, either because of the type of the stocks – Pelagic RAC, or the Long Distance RAC for the fleets that go beyond EU waters – or because of common management issues around particular areas – North Sea, Baltic Sea, Mediterranean Sea, North-Western waters, South-Western waters. This was a rather relevant aspect because the RACs' advice to the Commission was expected to seek consensus and, in this sense, the more the different stakeholders shared to begin with, the better in principle.

The emergence of the RACs can be illustrated with the first one created, the North Sea Regional Advisory Council. The records inform us that the NSRAC was created on the 4<sup>th</sup> of November 2004 in Edinburgh (NSRAC, 2004b). To understand why the NSRAC was the quickest to be established one has to attend to some earlier institutional developments in this particular region that paved an easier way than in other areas (NSRAC, 2004b; see also Degnbol and Wilson, 2008). The so-called 'North Sea Commission Fisheries Partnership'<sup>135</sup> (NSCFP) had been operating for a

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<sup>134</sup> Yet, when it comes to the environmental NGOs, it is noteworthy that a well-known organisation such as Greenpeace declined to join the RACs, as they did not feel that being a member of these *fora* could help anyhow their radical activism. Other less well-known organisations followed the same approach and decided to stay out of the RACs (see Griffin, 2013).

<sup>135</sup> Created by the North Sea Commission around the year of 2000 (see Degnbol and Wilson, 2008), the NSCFP was formed to gather scientists and fishing industry representatives from around the North Sea – therefore, not all stakeholders were included. The objectives of the initiative were to improve the exchange of views between fishermen and fishery scientists and to promote dialogue.

few years as a platform between scientists and fishing industry representatives in the North Sea to try to exchange expert knowledge about the fish stocks and to improve dialogue on management instruments (Hawkins, 2005). The NSRAC flourished from this existing collaboration between scientists and fishermen, and part of the rationale informing its foundation was that the experience of the fishermen could contribute to fisheries management, and inform decision-making as much as formal experts<sup>136</sup> (Degnbol and Wilson, 2008).

Notably, the process of institutionalisation of the NSRAC faced some challenges<sup>137</sup>. First, it was widely acknowledged that the NSRAC meetings needed scientific attendance but what the role of science exactly would be was less well established. The fishing industry within the NSRAC considered that the (new) role of science should be to support the RACs by assessing the ideas coming from the stakeholders – and, in so doing, contribute to solving the disputes between the different groups, and in particular between the fishing industry and the environmental NGOs (NSRAC industry representative-a, ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

A second important – and related – issue concerned the fact that the NSRAC advice should be based on consensus across its membership. The question then was whether the Commission should automatically stick to any NSRAC advice informed by consensus. In the view of DG-FISH, they would feel obliged to follow the NSRAC advice only if it was aligned with the principles of the new CFP. The industry within

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Furthermore, the intention was to make better use of information held by fishermen to improve stock assessments and to develop consensus views on fisheries management (Hawkins, 2005).

<sup>136</sup> At the same time, another important element of the rationale was to offer a platform for seeking consensus between the fishing industry and the environmental NGOs, which were not involved in the NSCFP (Degnbol and Wilson, 2008).

<sup>137</sup> I will not produce an exhaustive account of all challenges that the NSRAC was confronted with in its early days though. For further insights see Degnbol and Wilson (2008; see also Griffin, 2007, 2009, 2010, 2013; for a larger picture see also Linke et al., 2011, for the Baltic Sea RAC; Hegland and Wilson, 2009, for the Pelagic RAC).

the NSRAC was not very convinced by this argument as it was up to the Commission to establish what was in the direction of the CFP and what was not, thus undermining the *raison d'être* of the NSRAC (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

A third issue was how far the NSRAC should go as a negotiating partner with DG-FISH. There were voices within the NSRAC industry that supported that the national fishing organisations of each Member State were better suited to negotiate face-to-face with the Commission when it came to regulatory measures. These actors remarked that there were times where it was extremely challenging for the NSRAC to achieve a unanimous position given the diversity of actors and interests and they preferred to keep these difficult issues away from the NSRAC discussions. One of the issues where the national fishing organisations wanted to have direct consultation with the Commission was clearly on the level of TACs and quotas. In other words, in their view the NSRAC should deal with the definition of mid-term strategies but not with the annual negotiations over TACs and quotas (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

#### **4.3.2 Mixed fisheries**

The second major issue of the CFP reform was the shift from the single species fish stock to the whole fishery as the unit of analysis, which can encompass a mix of species. Mixed fisheries indeed named the core of the management problem that the 2002 CFP reform was willing to tackle. ICES had traditionally given fishery management advice on a stock-by-stock basis disregarding technical interactions when fishing between overlapping stocks or even non-commercial species. Yet many times more than one species end up in the net. And the problem for policy-makers came when the various species netted together required different degrees of management intervention – for instance, some of those species could be endangered and some not. Technical adaptations of the fishing gear were often flagged by policy-makers and scientists as possible solutions to the problem but, in general, the



fishermen disliked them due to their potential effect on their fishing success and revenue. All in all, trying to control the fishing mortality on each individual stock but also collectively across a mixed fishery was the second policy challenge that the policy-makers in Brussels wanted to address with the reform of the CFP.

Notably, there were only two fundamental ways of limiting the fishing mortality that could be considered: to regulate fishing output by means of quotas, or to regulate fishing pressure in terms of fishing capacity or fishing effort. Each system had its drawbacks; output control through quotas demanded monitoring all catches and struggled with how to take discards into account. Effort control raised, *inter alia*, the question of how to define parameters accounting for fishing capacity and monitor them with a standardised method. In rather pragmatic terms, this form of control was normally operationalised as a limitation to the number of ‘days-at-sea’ that the vessels were allowed to spend out of port for each vessel group<sup>138</sup>.

In addition, it is important to stress that TACs and days-at-sea did not blend well; these policy instruments were not meant to be used together (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). In the view of DG-FISH, effort control should replace in principle the TACs and quotas, but abandoning the latter was not an option due to the need to preserve the principle of relative stability. Catch quotas were an effective way to institutionalise each country’s size of the pie, and in this sense the mechanism worked well (Holm and Nielsen, 2004). And as a regulatory approach it was also more straightforward. At least everybody knew what a ton of fish landed was, whereas a unit of effort was much more difficult to agree on in the Council of Ministers and even more difficult to control because it was less transparent (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

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<sup>138</sup> For instance, different gear types or engine power meant the fishermen got a different number of allowable days per year (per fishery).

Yet, the blending of the two fundamental approaches was seen as the only way forward in practice. Relying mainly on TACs and quotas<sup>139</sup> was not enough when addressing the problems of the mixed fishery.

Combining control over the catches through quotas and over the effort through days at sea posed a new set of challenges to the policy-makers in Brussels, as follows:

We clearly perceive that we need to develop direct effort management, as strongly recommended by scientists [biologists], but at the same time we cannot do away with the catch limitations because they are the expression of the relative stability, so we end up inevitably with a duplication of instruments. And the relationship between these two instruments is basically, perhaps, subject to certain frictions from time to time. There's no clear hierarchy, which means in practice that [you can] run out of quota but you still have some fishing days available [...] Conversely, [you can] still have some quota left but you have run out of effort [...] So basically, the duplication means right now that the one that is exhausted first is the one that prevails (DG-FISH conservation unit representative-a, personal communication, March 2007).

However, this was only a rule of thumb and, in fact, one that was particularly problematic in those countries running an ITQ system, where the quotas were huge private investments by the vessel owners. At the time of research, this was for instance the case of the Netherlands, and the Commission had to develop a strategy for these particular cases:

It has been particularly important for a part of the [flatfish] industry, particularly for the Netherlands, to make sure that the application of effort limits did not basically result in the [privately owned] quotas not being caught. So we have gone as far as we could in trying to ensure this but, of course, the solution will require quite a lot of good scientific information to try to match the effort levels with the [quota] levels in a

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<sup>139</sup> With the addition of some other measures “mesh size limitations (including sorting grids), a minimum catching size, a maximum by-catch of undersized fish, maximum by-catch of non-target species, closure of areas with high densities of juveniles, and other seasonal and area restrictions” (ICES 2004b:4).

way that will make the two instruments as perfectly matching as possible, and this is technically difficult, but that is one of the challenges [...] we think [that it] can be done, but the idea is, in any case, that once those levels are estimated, we establish those levels in the relation and we don't modify them in the following year. In other words, the idea is to say that we do our best to try to make sure that effort levels and catch levels match, but this is not an excuse to start saying in the implementation year 'but well the match is not perfect, in fact with that effort level I will only catch 95% so I need more days'. No! (DG-FISH conservation unit representative-a, personal communication, March 2007).

As follows, matching fishing effort with quotas – and therefore with fishing mortality – was already expected to be the new matter of concern with the fishing industry (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). In this sense, the Commission was ready to exercise its authority in front of the fishermen, who could 'bend' the science for their own benefit, or so was the belief in Brussels:

A potential problem that may appear is that no matter how relatively clear the relationship can be between fishing mortality and fishing effort, fishermen always, and will always have a certain room for manoeuvre to try to use a given effort, to focus their fishing activity on certain areas and techniques that will maximise fishing mortality on some species and not in others. So that's why it is important for us to make sure that once we adopt this effort level and discussed level... sorry, if you fundamentally modify that relationship that's your problem. Don't come to us saying we need to modify that because otherwise that will simply render the whole system impractical. Basically, the system has to bite and be limiting (DG-FISH conservation unit representative-a, personal communication, March 2007).

Now that the challenge of managing mixed fisheries has been exposed it is time to move to the third regulatory change. In order to avoid short-term tactical decision-making, the CFP reform in 2002 also introduced long-term management plans, which will be thrown under the spotlight in the next subsection.

#### **4.3.3 Long-term management plans**

The reform of the CFP in 2002 pursued a shift from short-term policy tactical decisions year in year out to multi-annual management strategies (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). Long-term management plans consisted of a set of goals and an instrument to make those goals happen, the so-called harvest control rules. In essence, HCRs were meant to be applied every December for a number of years until the health of the fish stocks matched the policy goals set in the long-term plan. HCRs were supposed to work for the fishermen as well. Crucially, the degree of fluctuation in the TACs with respect to previous years was in most cases capped by the HCR in order to give the fishing industry some sense of stability – or progressive adjustment at least. The Commission's argument was that management plans had the advantage of providing the fishery with longer-term perspectives, economic, social and biological, thereby reducing the annual political turmoil around the TACs and quotas decisions (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). In this sense, at least in principle long-term management plans could offer the fishing industry a better framework to plan their investments. By and large, this shift from annual tactical decisions to more strategic decisions was fundamentally a change in the time frame but, at the same time, the same mechanistic rationale prevailed in the sense of establishing objective goals through the use of science.

Notably, before being implemented the HCRs had to be assessed by ICES. In principle, ICES scientists had to decide whether the HCRs proposed by DG-FISH were consistent with the precautionary approach or not. This was a new task that implied that a new approach to scientific advice had to be institutionalised in order to accommodate the shift to long-term management plans and the need to assess them. This new type of advice versed in what could be the plausible future scenarios for a whole fishery if a certain HCR was adopted; what would be the trade-offs between those different scenarios, or alternatively, given a set policy objectives, how could they be effectively achieved (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

In the fishing industry's camp, the mechanistic flavour of HCR did not produce much enthusiasm. All of a sudden HCRs were presented by scientists and policy-makers alike as the panacea to solve the pressing problems of the regulatory framework. In the view of the fishing industry representatives in the RACs things were being done in the wrong order; if the long-term management plans were to be successful the first priority should not be the biology but the achievement of better governance. For this reason, the RACs declared their willingness to take active part in the definition of the long-term management plans. In so doing, they expected not only to discuss the options when it came to the HCRs but also the goals, briefing DG-FISH with what needed to be considered (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

The RACs indeed adopted some 'red lines' with regard to long-term plans (see Pope et al., 2006). In their view, the goals of the long-term management plans should be adaptable and flexible – balancing the different pillars of sustainability, not only the biological aspects – and looking for directions of progress rather than fixed narrow long-term biological targets. Their message was heard in the Commission, as follows: "Both the fishing industry and the Member States' governments prefer not to tie up the hands, [they say] let's look at mid-term objectives and then we will have the time to think about long-term objectives. So there is a certain fear to tie everybody's hands to long-term objectives" (DG-FISH conservation unit representative-a, personal communication, March 2007). In the next subsection I will look at these objectives for the long-term management plans.

#### **4.3.4 Maximum sustainable yields**

Concurrent with the reform of the CFP, DG-FISH decided to adopt some of the policy objectives that had been earlier agreed in the environmental international arena. In 1992, the 'United Nations Conference on Environment and Development' (UNCED), also known as the first Earth Summit, was held in Rio de Janeiro. One of the hallmarks of this event was the agreement on the so-called Agenda 21, a plan for

translating the principle of sustainable development into action, particularly at the local levels. ‘Chapter 17’ of ‘Agenda 21’ was dedicated to the protection of the oceans, all seas, coastal areas and their living resources, aligning with the Law of the Seas and promoting the adoption of the precautionary principle in fisheries management. The latter effectively entered in force in 1995 when the ‘FAO Code of Conduct for Responsible Fisheries’ was published. Later on, in 2001, Iceland hosted the ‘Reykjavik Conference on Responsible Fisheries in the Marine Ecosystem’. This event focused on the acknowledgement that fisheries affect the ecosystem and that the ecosystem has an impact on the status and productivity of the fisheries resources. A request to develop guidelines for the inclusion of ecosystem considerations into fisheries management was issued in the final declaration. The response came two years later, in 2003, when the FAO published some technical guidelines informing of the ‘ecosystem approach to fisheries’ (FAO, 2003).

Meanwhile, in 2002 a second Earth Summit was held in Johannesburg under the name of the ‘World Summit on Sustainable Development’ – also known informally as ‘Rio +10’). The WSSD 2002 did not bring in new approaches or concepts but an agreement was reached to revisit the ‘maximum sustainable yield’ (MSY) approach<sup>140</sup> – which had already seen its heyday in fisheries management (Larkin, 1977) – and to encourage the ecosystem approach to fisheries. In this sense, a significant outcome of Johannesburg was the set up of specific deadlines for the maintenance or restoration of the fish stocks to biological levels that can yield the MSY no later than 2015 and the application of the ecosystem approach by 2010.

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<sup>140</sup> There had been a similar agreement at Rio in 1992, but little had happened. At Johannesburg, governments agreed to implement MSY (DG-FISH conservation unit representative-b, personal communication, March 2007).

In parallel with the reform in 2002, the European Commission embraced the political impulse to MSY agreed on in Johannesburg<sup>141</sup> (DG-FISH conservation unit representative-b, personal communication, March 2007). MSY implied moving away from a policy of avoiding short-term risks – i.e. taking precaution – to enlarge the biological and economic benefits in the long-term. Thus, as a policy instrument, MSY provided in principle a sound argument for change.

The rationale behind MSY has a straightforward formulation. Raising the fish stocks at the point where they allow for the highest catching opportunities without affecting their carrying capacity. At MSY levels the number of adult fish with capacity to spawn fluctuates less and this avoids boom and burst periods, resulting in more regular catches and greater economic stability for the fleets. In other words, and using a financial analogy, seeing the fish stock as a bank account, MSY amounts to withdraw only the interests. It follows that an MSY-based fisheries policy aims to maximise the interests of each stock. However, this is easier said than done. The MSY concept populates fisheries science textbooks, where its suggestion of equilibrium works with graphical beauty, but when it comes to implementation it is at best a moving target (Hilborn and Walters, 1992; Kell and Fromentin, 2006). It is difficult to know how much the maximum ‘interest’ is at a given time, to follow with the financial analogy. Furthermore, there is also another issue in relation to some of the mixed fisheries that can be found in the EU waters. MSY is, by definition, a single species concept and thus, is ill-defined for situations where what has to be managed is a mixed fishery, where each of the species might develop in different ways – sometimes even at the cost of the rest of species in the fishery.

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<sup>141</sup> Meanwhile, the implementation of the ecosystem approach was watered down, as follows: “Our approach is that moving individual stocks towards maximum sustainable yield is by itself a good contribution to improving biodiversity and reducing the human impact on ecosystems and a whole basket of other things which are part of the Johannesburg” (DG-FISH conservation unit representative-b, personal communication, March 2007).

In response to the challenges, the DG-FISH published in July 2006 some guidelines on how to operationalise the concept of MSY (European Commission, 2006a). Notably, instead of following Johannesburg to the letter by setting target stock size levels, DG-FISH proposed to reduce fishing mortality towards values that could lead to a MSY level of biomass. By and large, using fishing mortality as a proxy was their attempt to overcome the difficulties of defining MSY as a biomass equilibrium point:

Biomass is much less reliable in its evaluation or in its estimation than fishing mortality levels. And secondly, because of the changes in the ocean, including global warming but not only, the objective biomass that you can achieve is something that could change over time. So you could perhaps think of a biomass objective in the long run, you could say that this may turn out to be in the long run too optimistic or too pessimistic. Whereas, by fixing long-term objectives in terms of rates of exploitation [that is, fishing mortality] what you do is that, well, whatever nature will bring me, I will simply make sure that my rate of exploitation is sufficiently low to allow a maximum benefit from whatever nature can give me. And this is the focus, and this is really what we are trying to get through in all long-term plans (DG-FISH conservation unit representative-a, personal communication, March 2007).

However, biologists had problems as well when estimating the fishing mortality figure at which a single fish stock produces its maximum yield. In fact, they simply did not know and had to look for proxies that had more statistical than biological or socio-economic meaning (see European Commission, 2006a). Moreover, not only was the use of certain proxies an open issue but the discussion on how to implement MSY still had to take place formally in the Council of Ministers at the time of research (DG-FISH conservation unit representative-b, personal communication, March 2007).

For these reasons, the fishing industry within the RACs and some Member States expressed their concerns with the Commission's adoption of fishing mortality figure at which a single fish stock produces its maximum yield as the reference point to



establish long-term management goals, as it was the case with the flatfish fishery in the North Sea. Still the rationale had arrived to stay according to DG-FISH<sup>142</sup>:

European fisheries has for many years been driven by avoiding limits rather than by trying to be productive, [by] trying to get benefits from the sea. That has not been the driving force; it has really been ‘stay away from disaster’ [...] You could see the Johannesburg declaration in that way, as a way of saying now we can move back to start to look at targets and not only limits. So this is how it was picked up in here in the Commission. We say, look, we just see this as a soft political concept, it’s not the classical MSY concept but it’s a statement that we should move back to much lower exploitation levels where we have a productive sea. So this is how it was picked up and we made a communication on this last year, which does relate to the classical MSY concept, but that is mainly put there to make the point that we are far above any meaningful targets exploitation rate now and we must reduce, for most of our fisheries, we must reduce the fishing pressure substantially [...] So that’s the approach. And the political leverage from this is the Johannesburg declaration, so this is what the world is saying we should do and we are now interpreting this in this way (DG-FISH conservation unit representative-b, personal communication, March 2007).

Perhaps unsurprisingly, the environmental NGOs backed DG-FISH in their approach to MSY, as follows:

It was only because of the Johannesburg decision that the Commission had something... Okay yes [they] always wanted to go there but [they] couldn’t, at least that’s my interpretation, [they] couldn’t tell the fishermen why [they] had to go there. But now they say, yes we have this commitment to Johannesburg so we should go there because we’ve all decided that we should go there but apart from that it is a very good direction because it makes sense in economic terms and also in terms of ecosystem based management. That is what the Commission says and

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<sup>142</sup> Notably, the new reform of the CFP passed at the time of writing [summer 2013] now reflects a formal commitment to MSY objectives – in 2002 they were not part of the legal text of the CFP – by 2015 where possible and by 2020 at the latest (see European Commission, 2013a). It is also noteworthy that ICES revised in 2009 its advisory framework – based on doing precaution up to that point in time – and since 2009 ICES advice aims formally at achieving MSY through the application of HCRs (Lassen et al., 2013).

that is fine with me, what I think about MSY is that is of course a rather simplistic model but I think the way the Commission deals with it is very pragmatic and I think that is the good choice, to focus on the reduction of fishing mortality, that's the only thing they can do and what has to be done in the flatfish fishery [...] But still of course fishing industry is focused on ah! they are going for MSY and it is a discredited concept! (environmental NGO representative, personal communication, February 2007)

This brings to an end the account of the new issues introduced with the reform of the CFP in 2002 and the agreements of the WSSD in the same year, which complemented the CFP reform by offering DG-FISH the political leverage to pursue MSY yields for EU fisheries. DG-FISH was set for more openness towards stakeholders, mixed fisheries considerations, the application of long-term management plans and the adoption of MSY goals. As the argument goes, the depth of the regulatory reform was the reflection of how badly DG-FISH wanted to move away from the crisis picture in EU fisheries.

Yet a full account of the regulatory landscape is not yet complete. As I shall address in the next section, in 2002 the Secretariat General of the European Commission passed a strategy to improve the Commission's proposals for regulation – the so-called 'Better Regulation' strategy. As in any other Directorate General of the European Commission, the 'Better Regulation' strategy brought mandatory changes to how DG-FISH should come up with new pieces of regulation and this ended up having an effect on how the implementation of some of the new policy instruments introduced in 2002.

#### **4.4 The disembarking of the European Commission's 2002 'Better Regulation' strategy in EU fisheries management**

The reform of the CFP arrived in parallel to measures for improving the quality of EU regulation, which also deserve detailed attention. In June 2002 the European

Commission introduced the so-called ‘Better Regulation’ strategy as an attempt to “improve the quality of legislation, make its working methods more transparent and set the example of good practice” (European Commission, 2002a:5). The strategy followed from the UK Presidency of the European Council in 1998 (European Council, 1998) and successive ‘EU summits’. It received as well strong inputs from the European Commission’s ‘Sustainable Development Strategy’ (European Commission, 2001a), with its focus on the integration of economic, social and environmental considerations, and its white paper on ‘European Governance’ (European Commission, 2001b), promoting more involvement of the public in decision-making – both important considerations that already shaped the reform of the CFP in 2002.

While the discourses of better regulation have tended to change across different policy constituencies (Radaelli, 2007), the European Commission’s approach to better governance was originally conceived on the basis of a technical-rational model of decision-making by and large<sup>143</sup> (Hertin et al., 2009a; Radaelli, 2004). Thus, as the operational procedure for the practical implementation of the Better Regulation strategy, integrated impact assessments came into the Better Regulation in 2003 with the ‘Mandelkern Group’, an *ad-hoc* group of experts set to establish a common approach to the implementation of this strategy (Hellenic Presidency of the Council of the European Union, 2003). An IIA, which tends to have a quantitative character (see European Commission, 2005a), names an *ex ante* evaluation of the environmental, economic and social consequences of any new pieces of regulation, with the collection of factual information and use of expertise being integral parts of

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<sup>143</sup> As Hertin et al. characterise it:

This view of policy making contains a number of (implicit) assumptions about the policy process and the role of knowledge within it. For example, it suggests that: policies are designed to address specific problems or objectives; that impacts of planned policies can be anticipated with a certain degree of accuracy; that there is a central decision-maker who selects a policy option on the basis of expected net benefits; and ‘better’ information necessarily leads to more ‘rational’ policies (2009a:1186).

such exercises (European Commission, 2002c). In this respect, IIAs play two formal roles in the context of the European Commission's policy-making. The first role is to depoliticise the Commission's proposals by exploring a range of regulatory choices that have to be contrasted according to their social, economic and environmental scenarios of impact<sup>144</sup>. The second role is that of a communication tool whereby stakeholders can take part in decision-making by means of providing policy salient data; alignment with the principles of 'good governance', consultation of stakeholders is thus encouraged (European Commission, 2002d).

The arrival of the Better Regulation strategy to DH-FISH implied that there was a new obligation – effective from 2007 – to conduct integrated impact assessments for every draft regulatory proposal in order to offer sound options from which the final proposal could be selected (Meeting of the NSRAC Demersal Working Group in London, observation, October 2006). Notably, at DG-FISH some feared that this would increase the “costs of knowing” (DG-FISH scientific advisor, personal communication, March 2007). In fact, DG-FISH had already created in 2006 a new economic unit to take care of IIAs. This new unit had coordinative but no operational capacity and was meant to rely on STECF for the evaluation of the new long-term management plans from a socio-economic perspective.

All in all, not only DG-FISH had to fix the problems of EU fisheries, but also they had to do it ‘in style’, that is, following the mandates of the Better Regulation strategy. The combination of internal and external elements in the reform of the

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<sup>144</sup> At the same time, the Commission qualifies this as follows:

Impact assessment is an aid to decision-making, not a substitute for political judgement. Indeed, political judgement involves complex considerations that go far beyond the anticipated impacts of a proposal. An impact assessment will not necessarily generate clear-cut conclusions or recommendations. It does, however, provide an important input by informing decision-makers of the consequences of policy choices (European Commission, 2002b:3).

regulatory framework in 2002 meant an exceptional time in EU fisheries management. Things were bound to change – or were they? Commenting on the struggles with implementing regulatory measures, Griffin (2010) claims that EU fisheries management lives indeed on perennial ‘states of exception’ brought about by competing actors – the Commission, the Council of Ministers, the Member States, the fishing industry, and the environmental NGOs – all striving to achieve ruling authority. As the argument goes, there are always ‘good (authoritative) reasons’ – from time constraints to all kinds of contingencies – why agreed procedures in Brussels cannot be followed on the ground at a given point in time and end up being by-passed.

I will come back to this discussion once I have fully introduced the main case study over the design and implementation of a long-term management plan for the North Sea mixed flatfish fishery. Yet, beforehand, it is also important to reflect on the continuities. Things did not start to change immediately after 2002 (see Gray and Hatchard, 2003), and some of the issues seemed rather entrenched, as I will show in the next section.

#### ***4.5 The problematisation of the scientific stock assessments by the EU fishing industry beyond 2002***

In 2006, in the aftermath of the CFP reform of 2002, single species stock assessments were still locked in as the corner stone of scientific advice to policy and the fishing industry’s criticism to how ICES advice was produced continued. In this section, I will offer a glimpse of how the fishing industry problematised the stock assessments by reference to the empirical evidence concerning the ICES advice on the North Sea plaice<sup>145</sup> in 2006.

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<sup>145</sup> *Pleuronectes platessa* in its scientific denomination.

At that time, the North Sea plaice stocks were not doing well despite considerable scientific efforts. Over the years, a large number of biologists had considered that the decline of the plaice stocks was due to the high fishing pressure of beam-trawlers over the nursery areas (IMARES<sup>146</sup> biologist-a, personal communication, February 2007). In order to protect the nursing grounds for the plaice stocks, a squared area of special protection was established in 1989 following scientific advice from Dutch biologists. It was known colloquially as the ‘plaice box’ and the large beam-trawlers – of over 24 metres – were not allowed to enter this area, only the smaller beam-trawlers could access it. However, the introduction of the ‘plaice box’ did not revitalise the plaice stocks as expected by the scientists and raised more questions than answers within the fishing industry regarding whether fishing pressure was the main factor behind the poor condition of the North Sea plaice stocks:

Fifteen years ago they closed off areas of the North Sea as a closed area, or a limitedly closed area for plaice and they said this was going to be the nursery ground for a young plaice and once we do this we will have an abundance of plaice. That might have been a good idea in theory but [...] the good situation that they wanted to reach has never come. And that has made the fishermen very sceptical because they say, ‘now [scientists] say that if we reduce the fishing [pressure] the stock will increase [but] that will not happen because we are like farmers. If you do not plough the land, nothing will grow on the land. You need to plough the land to make it productive’ (Dutch fishing industry representative-a<sup>147</sup>, personal communication, January 2007).

Like other flatfish, plaice fishes dig themselves into the sand and the fishermen need to employ a gear capable of being dragged over the seabed to catch them. Fishermen considered this as a way of ploughing the seabed and nourishing the flatfish.

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<sup>146</sup> Acronym for the ‘Institute for Marine Resources and Ecosystem Studies’ in the Netherlands.

<sup>147</sup> This interviewee will be referred to as NSRAC fishing industry representative-b in Chapter 6. In this chapter I have left the country affiliation explicit because when discussing stocks assessments the fishing industry representatives mostly speak on behalf of their country.

DG-FISH had established rules in relation to the mesh size of the gear and the minimum landing size for the plaice species. At the time of research, above the 56 degrees northern latitude, the fleet – mainly Danish – could only fish with nets with mesh sizes of 120mm. In the area between 55 and 56 degrees – and west of the 5 degrees eastern longitude – the beam-trawlers could fish with 100mm mesh. Below this latitude, it was permitted to fish with a mesh size of 80mm, a gear effectively designed to catch another flatfish, the common sole<sup>148</sup>, smaller in size. Indeed, under the 55 degrees there were many fishing grounds where plaice and sole live together. These zones of the fishery were exploited by the Dutch fleet of large beam-trawlers of over 24 metres, sometimes using the British, Belgian or German flags, the so called ‘flag vessels’, owned by Dutch investors (Dutch Fish Product Board, 2008).

The lack of observable benefits that were originally expected from the ‘plaice box’ led the biologists and policy-makers to reframe the possible reasons behind the decline of the plaice stocks towards the amount of by-catch<sup>149</sup>. The Dutch fishermen targeted both sole and plaice but they were generally more keen on sole, of much higher value; and the side-effect of using a 80mm mesh size was the large by-catch of juvenile plaice below the minimum landing size. Therefore, the risks to conservation for the North Sea plaice stocks originated in the fishing for the rather slender sole due to this by-catch of undersized (juvenile) plaice, which was discarded back into sea because of the impossibility of landing it legally. Moreover, sometimes the Dutch fishermen would exhaust their quota to land commercially sized plaice while still having some sole quota left. In this situation they would also discard the ‘over-quota’ of plaice, even within legal size.

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<sup>148</sup> *Solea solea* in its scientific denomination and commonly known as ‘Dover sole’ in the UK – a misnomer according to Clover (2004), since at the long-gone heydays of the UK flatfish fishing industry the sole was most probably caught in the Dogger Bank and landed in Lowestoft.

<sup>149</sup> Yet a number of Dutch biologists considered plausible the thesis of the Dutch fishermen that beam-trawl fishing might increase the food availability for flatfish because of the ploughing effect (IMARES biologist-a, personal communication, February 2007).

Discarding was a common practice and, more importantly, a legal one at the time of research<sup>150</sup>. Because it happened off the record at sea, it was a problem for the scientists, who struggled to estimate discard numbers for their stock assessments. On the other hand, while some of the fishermen did not like the idea of discarding, many did not see much of a problem after all because the living creatures on the seabed would benefit anyway<sup>151</sup>:

They now say that the problem of beam-trawling, especially with this 80mm mesh, is that there is a lot of discarding, is not the impact on the seabed but the by-catching. The problem is the plaice from 15 to 25cm size that stay in the net but for me this is not a problem because it is a fertiliser for other species (Dutch fishermen, personal communication, January 2007).

In essence, for the fishermen the problems affecting the North Sea plaice stock were of a different nature. The fishermen believed in exogenous reasons to those involving the practice of fishing in order to explain the decline of the stocks. For instance, they were seriously concerned that climate change could be already altering the distribution of the fish by warming up the temperature of the water<sup>152</sup>. However, when they looked at the modelling process in the stock assessment exercises they did not see many of these concerns adequately reflected: “Managing stocks is not simply a case of increasing or decreasing the fishing mortality; many other factors besides

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<sup>150</sup> This will change with the new Common Fisheries Policy entering in force in 2014 (see European Commission, 2013a).

<sup>151</sup> As follows, although by-catch can be reduced by employing larger mesh-sizes the fishermen were mostly opposed (see also Groeneveld, 2011).

<sup>152</sup> Another external factor often cited by the fishermen to explain changes in the nature of the flatfish stocks in the North Sea was the following. Addressing in particular the long-lasting decline of the plaice stocks in the North Sea, some of the flatfish fishermen had turned their heads to studies that claimed that the reduction of the eutrophication of the North Sea might have had a strong effect in the food available to the stocks. To put it short, the flows of the rivers into the North Sea are much cleaner than they used to be, following the EU ban over the use of phosphates and certain fertilizers in agricultural production or the mandate for riverside industries to pipe cleaner water into the rivers. In this sense, the plaice stocks could have lost out if the 'polluted' water acted effectively as a fertilizer for the seabed too, resulting in less food availability overall. Some biologists believed that this was a factor among others for the reduction of the stocks (IMARES biologist-a, personal communication, February 2007).



fishing affect the size of the spawning stock – like climate change or pollution” (Dutch fishing industry representative-b, meeting of the NSRAC Demersal Working Group in London, observation, October 2006).

To illustrate this point further, let us look at the stock assessment for the North Sea plaice conducted in 2006 as it was discussed at a meeting of the NSRAC Demersal Working Group in London in October 2006, where representatives from ICES and DG-FISH were present. In order to rebuild the plaice stock to its precautionary levels in 2007, the ICES ACFM advice asked to reduce the TAC by 32,000 tons with respect to the previous year. This would reduce the current fishing mortality levels and, therefore, the risk of collapse. The assessment relied on discard information from some scientific monitoring of the fleet and fish landings data from the seaports’ records<sup>153</sup>. Furthermore, the landings data were fine-tuned with some scientific survey-campaigns, which had been taking place over the years in the same fishing grounds so that they could be comparable in scientific terms. However, these surveys were one of the main issues of controversy with the fishing industry, as follows:

Surveys [are conducted in the South of the North Sea] and [ICES scientists] have done them for several years [...] We have the fishermen feeling that the plaice is moving more to the North. Our question is that when you are measuring on the same place [...] I think you are estimating out because we see a strong improvement in the plaice stocks more to the North [...] maybe our Danish colleagues can say something about this if their quotas are being fully and easily fished. Why not survey in the Northern part of the North Sea? [...] [Otherwise] is just a question of putting the wrong figures in the model to get the wrong figures out, that’s quite simple in my view (Dutch fishing industry representative-b, meeting of the NSRAC Demersal Working Group in London, observation, October 2006).

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<sup>153</sup> The fisheries are controlled, *inter alia*, by inspections of all fishing vessels when landing the fish. Keeping a detailed fishing logbook on-board is mandatory for most vessels, and large parts of the fleet report to the authorities on a daily basis (ICES, 2003).

Further, the treatment of the discard information was another problem in the eyes of the fishing industry. Consideration of discards data was a rather new thing in the stock assessment for plaice – before 1999 scientists did not have observations on an annual basis. The existence of large amounts of discards was a significant problem because the total fishing mortality could remain high – thus triggering large reductions in the TAC – despite a decrease in the fishing mortality corresponding to the landed fish for human consumption. From 1999 onwards, a modest number of discard surveys had been held every year by the Netherlands and UK biologists since these were the fleets more likely to discard plaice given that they target the valuable sole in the mixed flatfish fishery in the southern grounds of the North Sea. Yet other fleets that also fished for plaice, namely the Danish, raised their voice against the scientific procedure in the following terms:

I would like to add that it is totally unacceptable that these [discards] estimates are only coming for the Dutch and UK samples. We have from Danish industry year on year the data on the discards of the Danish flatfish fisheries [...] [do] you calculate the discard factor with the Dutch and UK data and then raise all the plaice landings [including the Danish] with the same factor? [...] I think is very important because I think that you raise the total landings from plaice with a discard factor from the Dutch 80mm beam-trawling fishery and that's is completely flawed [...] So I think there is something fundamentally wrong with this assessment on plaice and I think is really not acceptable [...] we cannot have [from the point of view of] the Danish fishing industry an advice to further reduce the TACs (Danish fishing industry representative<sup>154</sup>, meeting of the NSRAC Demersal Working Group in London, observation, October 2006).

ICES response was that the Danish information on discards was presented in a different format than the information from England and the Netherlands and, for this reason, it could not be used with the existing methodology. Yet, ICES acknowledged *in situ* the problem with their calculations by stating that, “with regard to the discard

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<sup>154</sup> This informant will be referred to as NSRAC industry representative-b in Chapter 6.

issue I think there have been valid comments around the table for plaice. This is the assessment we have been able to come up with so far but I will ask some of the issues raised here to be considered” (ICES representative, meeting of the NSRAC Demersal Working Group in London, observation, October 2006).

It is worth noticing that, given the problem of diminishing stocks, the environmental NGOs concerned with the conservation of the fish stocks also raised criticism but in a different direction, to what they saw was not poor scientific advice but poor governance. The environmental NGOs complained that the TACs advised by ICES were always watered down by the Member States at the Council of Ministers every end of December (environmental NGO representative<sup>155</sup>, personal communication, February 2007). If the TACs had been set following the biological advice, there would have been a good chance of returning to precautionary levels. However, TACs and quotas had often been set higher than the advice and, consequently, the stocks had enjoyed little or no opportunity to recover. Following pressure from the fishing industry over the December Council of Ministers, the TACs proposed by the Commission on the basis of ICES and STECF advice were considered not strong enough evidence to be used for the restriction of fishing at the Council of Ministers. Lack of long-term strategic ambitions was a big part of the problem, as follows:

We have already for years been advocating to the European Commission that their policy is basically missing something because they only have the limit reference points, the precautionary reference points but there was no target reference point so the policy, the quota would always focus on risk avoidance rather than trying to get a sustainable stock (environmental NGO representative, personal communication, February 2007).

All in all, not much seemed to have changed in practical terms in 2006, at least when it came to the stock assessments for the North Sea flatfish stocks. While this was

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<sup>155</sup> This informant will be referred to as NSRAC environmental NGO representative in Chapter 6.

only one particular case, the significance of the North Sea flatfish fishery could raise more general doubts over the real impact of the CFP reform. Yet the fact was that in parallel to these stock assessments things were slowly moving towards long-term management plans. As I will show in the empirical chapters, this was overall a policy process marked by the tension between change and continuity.

#### **4.6 Conclusions**

In this chapter I have first drawn on the scholarly literature to contextualise historically the advent of the Common Fisheries Policy in 1983 and describe its ‘machinery’. Then I have addressed the regulatory changes – internally and externally conceived – that DG-FISH introduced in 2002 in response to a crisis picture in the management of EU fisheries. I have informed as well as the arrival of the Better Regulation strategy to DG-FISH at the time of research, that is, between 2006 and 2007. The practical consequence of this was the irruption of the integrated impact assessment as a new policy instrument by which the new long-term management plans had to be evaluated on the basis of biological and socio-economic merits.

By and large, the rationale for the new regulatory instruments instituted in EU fisheries management was that policy decisions had to be made on the basis of more comprehensive scientific advice and without totally screening out the stakeholders of its production. Ultimately, the goal of DG-FISH with the reform of the CFP was to finally automatise the decision-making process over the fishing quotas on the basis of socially robust long-term management strategies, moving away from political bargaining every December at the Council of Ministers. Taken altogether, the different instruments introduced with the reform in 2002 represented the ‘reframing’ of the Common Fisheries Policy – i.e. the efforts to ‘cool it down’ (Callon, 1998). Furthermore, in coming up with long-term management plans and HCRs to strengthen the TACs, DG-FISH had now to anticipate a range of impacts for

different policy options and open up this assessment to stakeholders by virtue of the Better Regulation strategy. Therefore, it was not just about reframing the CFP but also about doing it ‘in style’, with better governance. Once the management plans with their harvest control rules therein were approved, the TAC Machine could finally ‘cool off’ on the basis of a clear-cut division of labour between the producers of the advice and the decision-makers, or so was the general ambition.

This chapter will serve as the backdrop to a case study of the establishment of a long-term management plan for the mixed flatfish fishery in the North Sea. Using this empirical case, in the following three chapters I will argue that, far from automatising decisions free of political considerations, the regulatory changes introduced enabled new spaces for political work. In other words, spaces characterised by the tension between depoliticisation and politicisation, cooling down and heating up debates, and also tension between change and continuity of governing rationales.

## **5 Building a new framing for the North Sea flatfish fishery: The introduction of integrated assessment modelling**

### **5.1 Introduction**

In the previous chapter I addressed the set of policy innovations introduced in EU fisheries management at the turn of the 21<sup>st</sup> century. They were all constituents of a process of reframing, that is, of the building of a new framing to enable EU fisheries management decisions, as I shall illuminate by means of a case study of the mixed flatfish fishery in the North Sea.

DG-FISH selected this fishery to be the first to undergo the establishment of a long-term management plan with a mixed fisheries approach *and* MSY as a long-term objective for this plan<sup>156</sup>. Later it also ended up being the first one where an *ex-ante* integrated impact assessment was used to legitimise the long-term management strategy. In this sense, the case represented a unique opportunity to follow the introduction of the IIA, which different actors – including those in the scientific advisory arena – wanted to domesticate.

The case study will span across three chapters including this one, mirroring what I have defined as the troika of instruments. In this chapter I will throw the spotlight on the computer modelling mobilised for the IIA, commonly referred to as ‘integrated assessment modelling’ (IAM). The modelling opened up a space for the reshaping of the scientific advisory arena supporting DG-FISH. As I will describe, DG-FISH appointed a group of EU biologists and economists under the aegis of the STECF to

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<sup>156</sup> The flatfish fishery management plan was the first and only one to have maximum sustainable yields as an explicit objective in the management plan at the time of research. Indeed, at that point, the implementation of MSY approach was still pending discussion at the Council of Ministers and only a communication paper by DG-FISH had been released.

conduct the IIA. I will claim that the IAM efforts helped – if only modestly – to overcome the division of labour between biologists and economists that had previously characterised the advisory arena and led to the emergence of a more interdisciplinary space for knowledge production.

Indeed, to produce any ‘integrated’ evidence for policy decisions the necessary condition was to redistribute the authority between fisheries biologists and economists in the scientific advisory arena. In this sense, I will show how the modelling activity became more a strategic space for such redistribution than an instrument to represent what was happening in the fishery.

The structure of the chapter is as follows. In §5.2 I will describe the existing division of labour between fisheries economists and fisheries biologists in the provision of advice to EU fisheries managers before the reform of the CFP in 2002 – and right after. In §5.3 I will address the strategy proposed by DG-FISH after the reform to manage the flatfish fishery in the North Sea, that is, the long-term management plan for the sole and plaice fisheries. As I shall explain, this proposal triggered a request for an IIA exercise from the Flatfish Fisheries Working Group in the NSRAC<sup>157</sup>, which not only questioned whether the long-term management plan would make any significant difference in terms of the fish, but also voiced fears about its socio-economic repercussions on the fleet. With the support of other stakeholders in the NSRAC, and especially their Danish counterparts, the Dutch fishing industry asked DG-FISH to produce sound evidence that the plan would work in both biological and socio-economic terms. Meanwhile, the Dutch government made a parallel request, mounting pressure to the Commission call for the implementation of the IIA instrument in the context of the long-term management proposal for the flatfish. In their view, DG-FISH should have launched an IIA before releasing the management

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<sup>157</sup> Led by the Dutch fishing industry.

proposal for the flatfish, and in failing to do so, had missed a great opportunity to show that they were serious about improving governance. As I will describe, DG-FISH finally agreed to run a formal IIA, which took place over two workshops. In §5.4, I will consider the first workshop. To begin with, I will outline the organisational arrangements that shaped the first workshop. Second, I will describe the modelling approach mobilised for the exercise. And third, I will introduce the challenge to the traditional division of labour faced by the biological and economic modellers as a result of the regulatory reform. In the event, the first workshop did not deliver satisfactory results, and a second workshop was commissioned to fix the problems. In §5.5 I will in turn discuss this second workshop. First, I will explain the rationale for the second workshop in detail. Then, I will deal with the new organisational arrangements shaping this second meeting. Third, I will examine how the modellers deconstructed the models, simultaneously overturning the existing division of labour. Fourth, I will illuminate how they moderately succeeded in both re-shaping the scientific advisory arena and producing interdisciplinary knowledge. Finally, in §5.6 I will offer some conclusions.

## **5.2 *The linear division of labour in the EU fisheries advisory arena***

As in the linear progress of an assembly line, a sequential division of labour between fisheries biologists and fisheries economists had governed the scientific advisory arena under the Common Fisheries Policy from 1983 until the regulatory reforms of 2002. This division of labour was hierarchical, with the biologists clearly ‘on top’ of the production of policy advice and the economists rather ‘on tap’ – i.e. called in only at the end of the advisory process each year to add some economic projections to the TACs figures that had been produced by the biologists. The interaction was therefore largely one-way, with the two sets of actors divided along disciplinary lines. By and large, the EU fisheries advisory arena presented a mechanistic arrangement, following Burns and Stalker (1994[1961]). Yet, as I will argue below, the introduction of long-term management plans and integrated impact assessments



after the reform in 2002 challenged this linear division of labour in the scientific advisory arena.

### **5.2.1 The shifting role of the ICES biologists in the EU fisheries advisory arena**

In Chapter 4 I discussed the role of ICES biologists and one of their key duties, the annual stock assessment. The provision of this short-term advice granted them authority in the scientific advisory arena. However, the demand to evaluate long-term management plans for mixed fisheries where TACs and effort limitations were combined was a job of a different kind for the ICES biologists working in the EU.

In this new context, ICES biologists turned to the production of ‘Management Strategy Evaluations’ (MSEs), that is, computer simulation exercises that allowed for evaluation of alternative management strategies and biological and socio-economic trade-offs between several management objectives<sup>158</sup> (Aranda and Motos, 2006). MSEs were a considerable challenge for the ICES biologists, who found themselves under somehow more ‘organic’<sup>159</sup> (Burns and Stalker, 1994[1961]) conditions; when working on these evaluations, the biologists were far from performing fisheries biology in the strict sense of being ‘spokespersons’ of the fish. Indeed, MSEs somehow entailed a shift from fisheries science to ‘fisheries management science’, meaning that the focus moved to the evaluation of fishery management choices as opposed to the traditional fish ‘counting’ (DIFRES biologist<sup>160</sup>, personal

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<sup>158</sup> In this sense, I conflate MSEs with IAMs throughout this chapter and the next one.

<sup>159</sup> Burns and Stalker characterised their notion of organic systems – which they opposed to ‘mechanistic’ systems – as follows:

In organic systems, the boundaries of feasible demands on the individual disappear. The greatest stress is placed on his regarding himself as fully implicated in the discharge of any task appearing over his horizon, as involved not merely in the exercise of a special competence but in commitment to the success of the concern’s undertakings (1994[1961]:103).

<sup>160</sup> ICES biologist affiliated to the ‘Danish Institute for Fisheries Research’ (DIFRES).

communication, March 2007). As I will show below, suitable modelling tools to carry out MSEs for long-term management plans had yet to be developed<sup>161</sup>.

In this new context, the biologists needed a much larger amount of data and, in particular, information about the behaviour of the fishermen at sea. Yet they lacked effective procedures to collect this data; therefore, their normal practice of examining the past to establish empirical relationships and project them towards the future was no longer feasible. Instead they had to turn to scenario-building for their MSE exercises<sup>162</sup>: “So we have presumed some extreme different scenarios [on] how our fishermen could behave [...] [trying] to find out how sensitive [the MSE exercise] is to one and the other extreme” (IMARES biologist-b, personal communication, February 2007). At the same time, MSEs were meant to involve fisheries managers and stakeholders, as well as economists and other experts, in their exploration of policy options – following a broader trend whereby ICES, and similarly STECF, had to be as transparent as possible (DG-FISH, 2007b).

When it came to their interaction with the fisheries economists, the linear division of labour that had long prevailed in the advisory arena under the CFP of 1983 was still dominant at the moment of introduction of the regulatory reforms in 2002:

[Biologists can provide an answer to] in what direction [a management option] develops and whether the [biological] consequences are minor or substantial and you need that anyway to do any further evaluation because you cannot do an economic evaluation if you don't know how the catches and the stock will develop; because an economic evaluation

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<sup>161</sup> At the time of research the work was being carried out under the European Commission's 6<sup>th</sup> Framework Programme.

<sup>162</sup> It is also noteworthy that the number of biologists with skills to meet the new challenges was rather small and that they felt considerably overwhelmed, thereby compromising the quality of the advice (DIFRES biologist, personal communication, March 2007). In addition, ‘trans-scientific’ questions (Weinsberg, 1972) – i.e. questions that have an apparently scientific formulation but which cannot be answered by means of science – were often put to these scientists, to the point that they felt that they were not doing proper science (Wilson and Hegland, 2005).

is partly cost, and that is [fishing] effort, and revenue, which is catch. [...] [And] the economists, they need to find whether [the management plan] brings in a stable situation [for the economy of the fleet] or a very fluctuable [sic] situation and on what level (IMARES biologist-b, personal communication, February 2007).

Until the arrival of IIAs and long-term management plans, the ICES biologists had been responsible for the calculation of the annual TACs, on the basis of which the economists had been forecasting the economic performance of the EU fishing fleets for the following year. As evidenced above, the ICES biologists considered this linear division of labour with the economists adequate to the new context. Let us now look at the role and perceptions of the EU fisheries economists.

### **5.2.2 The shifting role of the fisheries economists in the EU fisheries advisory arena**

Fisheries economists in the EU were highly sensible of the division of labour between biologists and economists in the advisory arena: “In the north-western area of the European Union, you have on one side the ICES biologists, and then you have the group of economists doing their work, and these two groups are more or less different. They are working on their own directions” (LEI economist-a<sup>163</sup>, personal communication, March 2007).

Before the arrival of IIAs and long-term management plans, these fisheries economists worked to estimate the economic performance of the EU fishing fleets in the coming year, given the proposed TACs and their average economic performance over the most recent years. To answer this question the fisheries economists applied the EIAA model – ‘Economic Interpretation of the ACFM Advice’ – immediately after the publication of the TACs by the ICES Advisory Committee on Fishery

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<sup>163</sup> This fisheries economist was affiliated to the Dutch ‘Agricultural Economics Research Institute’ (LEI for its acronym in Dutch).

Management. Notably, the bulk of their advice to the European Commission under the applicable regulatory framework consisted of calculations of costs and benefits.

It was a mechanistic arrangement of the advisory arena, as mentioned earlier. ICES biologists had been the dominant actors in the production of short-term advice in the EU and the reasons were clear in the mind of the fisheries economists, as follows:

Let's say [that during] the Second World War there was hardly any fishing. I only know about the North Sea but it was a quite dangerous place to go out with a boat during the Second World War, so there were hardly any fisheries [...] By that time ICES was already there so these people were getting together and they were putting those numbers together but it was not really used in management at the time. But then suddenly problems in fisheries came up [...] what do politicians want? They want an answer and some space around it to manoeuvre, because that's what politics is about. Now, basically the question to the biologists is quite simple. How many cod are there left in the North Sea? But those biologists don't answer the question straight, they don't say five. They say no, if you do this, it's five and it will go this direction [and] if you do this, it's five [but] it will go this [other] direction. So they are already presenting management options. And then, of course, the managers and the politicians say, welcome, tell me more, tell me more, because it's the biologists saying that it's like this and they can take the decision and can say 'because it's the biologists telling us that we need to do this, so if at all you want to be angry at somebody go see the biologists.' And that's how they entered centre stage. And that's how fisheries economists have always been lagging behind because they were not invited to the stage, because there was nothing like economics around. It was only about how much fish is there and which management options do we have (STECF economist<sup>164</sup>, personal communication, March 2007).

As follows, the scientific tools developed by both sets of actors mirrored somehow the hierarchy in the division of labour. The fisheries economists working in north-

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<sup>164</sup> At the time of research STECF formally enrolled between 30 and 35 expert members selected by the Commission in the fields of (*inter alia*) marine biology, marine ecology, fisheries science, fishing gear technology, aquaculture, and the economics of fisheries and aquaculture. Notably, the latter tended to dominate in numbers (LEI economist-a, personal communication, March 2007).

western Europe admitted the simplicity of their models. In the established division of labour the economists simply did costs and earnings accounting, so they did not need very sophisticated tools (LEI economist-a, personal communication, March 2007).

Yet the arrival of the Better Regulation strategy and the need to evaluate long-term management plans presented an opportunity for the fisheries economists to challenge the linear division of labour in the production of advice and play a bigger role in the advisory arena:

[The biologists] have a hundred-year history. They are really very, very strong ICES biologists, and they have a very good working system, advice system, and it was for economists not so simple to cooperate with them in the beginning [...] And there was, in the views of the biologists, not a direct need to include economy. This need has grown with the management system as soon as management changed to effort-driven system or limitations on sea days and [...] ways of managing fisheries on more than one species together. And then, all the old, hundred-years-old or tens-of-years-old models of the biologists were not complete enough. So and then the need for economic input was there. But that's only for, for, how long is that? Five or ten years or so [...] [Before that, biologists and economists] were forced to work together more or less [...] Let's say that some people of DG-FISH said economy should be included and also the European parliamentarians said that, I think since 1990 [...] but this was more the, well, 'window dressing' [...] But now with the changed management system, the shift to input management, now the biologists realise that their models are incomplete. They are unable to [advise] whether you should focus with your management plan on this species or that species or on both, or on a third species. To them all species are equal, except the cod [laughter] [...] so it's the way you have the organisation of management that has enforced collaboration between the two disciplines (LEI economist-a, personal communication, March 2007).

As follows, with the introduction of long-term management plans and IIAs, the economists also found themselves under a somehow more organic context and their tools were open to evolve to a higher level of sophistication:

The [real] complexity is [the] assessment of [the] behaviour of fishermen. As soon as you apply to the assessment the influence of fishermen behaviour or changing [in] their behaviour as a response to other management systems, then you have [...] a very complex domain and we have not achieved very much there in my view, so far (LEI economist-a, personal communication, March 2007).

What is more, the economists were willing to speak for the fishermen in front of the biologists and policy-makers, as they considered that fisheries management was too much focused on the fish stocks, with the fishermen conceived of as mere proxies to control the fish:

Usually my opening joke in all those talks is ‘have you ever taken a close look at fish, they don’t seem to have ears, so that’s why we start shouting at the next best thing with ears, that’s fishermen. But actually what [the managers] want to do is ‘fish [stock] management’, but fish don’t listen so we start managing the fishermen, but that’s not the same thing (STECF economist, personal communication, March 2007).

In summary, with the introduction of long-term management plans and IIAs, the linear division of labour between biologists and economists in the production of scientific advice to policy became undermined. I will dedicate the rest of this chapter to unpacking this process of change using the case of the IIA for the long-term management plan proposed by the DG-FISH for the North Sea flatfish fishery.

### ***5.3 The long-term management plan for the North Sea flatfish fishery and the request for an integrated impact assessment***

How did the regulatory changes described in Chapter 4 apply to the case of the North Sea mixed flatfish fishery under study? In line with the reform of the CFP in 2002, on 10 January 2006 the DG-FISH conservation unit published a new management proposal for establishing a management plan for fisheries exploiting stocks of plaice and sole in the North Sea (European Commission, 2005b). Plaice and common sole

are part of the larger family of flatfish – together with other species such as halibut, flounder, dab and lemon sole. Plaice is diamond-shaped with characteristic orange dots along the skin, while sole is a considerably smaller, tongue-shaped flatfish with brown spots. There is also a significant difference in the spatial distribution of the two species across the North Sea. Plaice is widely spread over the North Sea. Juveniles occupy the southern part, from which they migrate to the northern waters, where adults are to be found. Meanwhile, the adult sole lives only in the warmer southern waters of the North Sea below 55 degrees of latitude. At the time of research, the health of the two fish stocks differed as well. In 2003, just after the reform of the CFP, the biomass of the sole stocks was above the precautionary levels according to ICES, while in the case of the plaice stocks the situation was more worrying. The plaice stocks were below the threshold at which ICES biologists believed that the risk of collapse becomes significantly high (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

In response to the state of the two main flatfish stocks in the North Sea in 2003, DG-FISH started to work on a long-term strategy to bring the plaice and sole stocks to MSY levels by 2015 – in line with their ambitions. From the outset the plan proposed in 2006 addressed the two stocks as a mixed fishery (European Commission, 2005b). At the core of the new management strategy was a purpose-built harvest control rule. Conceived as a combination of TAC and effort days, the HCR was designed to ensure that the fishing mortality of plaice and sole could decrease steadily at the rate needed to reach MSY levels by 2015<sup>165</sup>.

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<sup>165</sup> The central elements of the HCR for the North Sea mixed flatfish fishery spread over three articles in the Commission's proposal: article four, article five and article six (see European Commission, 2005b). The HCR set target fishing mortalities by 2015 for plaice of 0.3 (article four) and for sole of 0.2 (article five), both values at which the Commission expected high yields according to scientific advice. The fishing mortality in both cases should go down at a rate of 10% per year until those targets were met. The annual TACs should be calculated according to the 10% reduction of fishing mortality ambitioned. However, fluctuation of the single stock TACs between years should not exceed 15% in order to provide the fishing industry some sort of economic stability – barring that the given stock was not declared at risk. Meanwhile, the output limitations (TACs) would be reinforced with input limitations as well (days-at-sea). In order to bring the fishing mortality down to those levels,

By and large, the Commission's management proposal was problematised by the NSRAC industry representatives and the Dutch government – the Netherlands being the Member State with the largest interests in the fishery. The days-at-sea limitation that DG-FISH wanted to introduce was a major matter of concern among the major players in the fishery – namely the Dutch flatfish fishing industry – because they interfered with the existing ITQ system operating in the Netherlands for this fishery<sup>166</sup>, as follows:

The thing is that you have to choose between an output driven system and an input driven system. If you think that the output driven system is not good you have to do it in a fair way and those who have the rights under the old system must have the rights under the new system (NSRAC industry representative-b, personal communication, January 2007).

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article six stated that the fishing effort should be cut down in the same proportion as the reduction needed for fishing mortality. Notably, DG-FISH had borrowed this linear assumption from fisheries biology, which should not come as a surprise since the policy-makers in conservation unit at DG-FISH were biologists by training in most cases and they approached issues accordingly (DG-FISH conservation unit representative-b, personal communication, March 2007):

[You] use a normal scientific procedure, you use Ockham's razor. You take the simplest assumption which is reasonable, you look at the data. If your data tells you that you have to do something else then you do something else. If you have complicated assumption without data I don't think you're going the right way.

<sup>166</sup> Already with the arrival of the new oceans regime in 1977 the Dutch fisheries administration introduced a system of ITQs per vessel to allow their fishermen to sustainably self-manage flatfish catches in the North Sea (Venema, 2001). This was in line with a long tradition of free enterprise of the Dutch high-sea fleet owners. The steady increase in fishing effort over the years led the Dutch fishermen to overfish and land illegally, so in the early 1990s the Dutch government stepped in and set a system of co-management between the state and the industry to set the quotas. In 1992 emerged the so-called 'quota management groups', known as 'biesheuvel groups', which had no equivalent at the time in any other EU country. Fishermen come together under these groups and delegate their right to manage their individual ITQs to a board of directors who ensure the collective commitment with the overall quota limitations for the whole fleet set in Brussels. In so doing, the board of directors, for instance, sets rules for the time spent at sea and allows the renting or bartering of quota within the group. Presided always by someone outside the fishing industry, the biesheuvel groups are considered a successful instrument to stop overfishing of the quotas in the Netherlands by partial delegation of management (Lequesne, 2004).



In other words, the problem was that fishermen who had invested a large amount of capital to buy the rights to fish under the ITQ system were now confronted by limitations in the number of days available to catch their quota.

At the time of research the Netherlands was the only Member State with such an internal arrangement for the distribution of fishing rights among some of their fishermen, therefore it was primarily a Dutch problem<sup>167</sup>. This might help to explain the fact that the Dutch government and the Dutch industry representatives at the NSRAC were the most eager to see evidence that the long-term management plan for the flatfish fishery proposed by DG-FISH would work and that their fishermen would profit from its implementation. Although an evaluation had been provided by DG-FISH in the memorandum of the long-term management plan proposal in January 2006, it had only cursorily considered the impoverished state of the fish stocks and the fleets before proclaiming the conservation and economic benefits of bringing down the fishing mortality, without sufficient further analysis:

When preparing regulations it's normally good practice to carry out an *a priori* evaluation of what effect those regulations will be [...] to give some background so that the decisions aren't made completely in the dark. Now it hasn't been good practice in fish stock management to do that until recently and there are two reasons. The first one is an impact assessment depends heavily on an economic and social analysis and the provision of economic data in European Commission on fisheries is very weak [...] no standard data collection exercise system exists concerning economic parameters in Member States. For biology things are different. There's a centralising institution, ICES, which establishes working methods for the different laboratories, the different national laboratories to work together. And using national funding in this framework of international co-operation we have developed standard protocols for the collection of biological data. In the economic world this does not exist [...] No international organisation to standardise the data collection, no framework to hold it together and no long-term funding to maintain long-

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<sup>167</sup> The Danish had also a very specific interest in the North Sea plaice stocks but "in most cases, an advantage to the Dutch will also be an advantage to the Danes in flatfish fisheries" (NSRAC industry representative-b, personal communication, April 2007).

term data collection. So we have a fundamental problem of getting economic advice because of that. And the second one is, because we work on conservation issues, the main risk we are trying to avoid is the collapse of fish stocks. And there the impact is, in brutal terms, when you look at the biology, fairly simple. If the stock was going to collapse the whole sector would lose its economic activity. So you have to take measures to avoid the stock collapsing. It's very, very simple (DG-FISH conservation unit representative-b, personal communication, March 2007).

Therefore, DG-FISH was firmly convinced that the situation had to improve first in biological terms by considerably reducing the fishing mortality, and only subsequently in socio-economic terms. Yet the NSRAC was far less convinced and, led by the Dutch fishing industry, demanded an integrated impact assessment without delay. They had reasons to be sceptical given earlier disappointments in the recent history of EU fisheries management, for instance with the 'plaice box' described in the previous chapter or the 'cod recovery plan'<sup>168</sup> (NSRAC industry representative-b, personal communication, January 2007).

Mounting pressure, the (former) Dutch 'Ministry of Agriculture, Nature and Food Quality' – LNV for its acronym in Dutch – also demanded that DG-FISH run an IIA in order to produce evidence that, in compensation for the pain of reducing the Dutch flatfish fishing capacity, the plan would indeed achieve its goals and leave the remaining Dutch fishermen better off. In their view, an integrated impact assessment of the policy plan should be carried out before submitting the proposal to the Council of Ministers (LNV representative, personal communication, January 2007).

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<sup>168</sup> At the time of research, this was a rather controversial piece of regulation aiming to bring this iconic demersal species of the North Sea back into shape. The regulation had very mixed and inconclusive results despite being quite restrictive on the catch of cod, whether as target species or as a by-catch, as in the case of the flatfish long beam-trawlers (Symposium on Cod Recovery in Edinburgh, observation, March 2007).

The DG-FISH conservation unit accepted these demands. It has to be said, nonetheless, that at the beginning of 2006 DG-FISH felt under no obligation to conduct a formal impact assessment. The Better Regulation strategy dictated that IIAs were required only for a particular list of proposals across the whole European Commission – the so called ‘Commission Legislative and Work Programme’ (CLWP) – which did not feature the flatfish long-term management plan (DG-FISH legal unit representative, personal communication, March 2007). Yet, when the conservation unit at DG-FISH received the requests from the Dutch government and the NSRAC, the adoption of the Better Regulation strategy was being internally discussed with the new economic unit, as well as with the legal unit, and the long-term plan for flatfish fishery offered the opportunity for a pilot IIA exercise to be conducted at STECF.

At this point in the chapter I have introduced three key elements. First, the proposal by DG-FISH for a long-term management plan affecting both the sole and the plaice fishery – the core of this proposal was the HCR. Second, I have accounted for the problematisation of the proposal by two actors – the NSRAC and the Dutch government – paying attention to their rationales. These actors managed to secure an integrated biological and socio-economic evaluation of the long-term consequences of applying the plan. Third, I have reflected on the motivation of DG-FISH to agree to run an IIA, an exercise that finally required two separate workshops, as I will discuss separately below.

## **5.4 The first workshop of the integrated impact assessment**

### **5.4.1 The organisation of the first workshop**

To start the ball rolling, DG-FISH established an *ad hoc* expert working group within STECF “to do their best possible job” (DG-FISH conservation unit representative-b, personal communication, March 2007). Its coordination and supervision was shared between the economic unit at DG-FISH as the new section created specifically to

lead the development of IIAs within DG-FISH, the Joint Research Centre (JRC) of the European Commission as the organisation that held the STECF Secretariat, and an independent chairperson. I will refer to them hereafter as the ‘convenors’.

The scope and depth of integrated impact assessments were meant to be governed by the so-called ‘principle of proportionality’, which implied that the required level of quantification depended on the policy relevance of the proposal (DG-FISH, legal unit representative, personal communication, March 2007). The more important a proposal for regulation, the more its potential impacts should be quantified. In the case of the IIA for the flatfish fishery, it was taken for granted that some form of modelling would be used to explore the various impacts. However, a standardised modelling approach to conduct this task had yet to be established. This offered the opportunity to the conservation and the economic units at DG-FISH to influence what sort of modelling approach was to be mobilised.

Notably, there were some differences in the approaches of the units. The conservation unit, staffed by biologists by training, advocated a sophisticated modelling approach, as close to reality as possible:

In the fisheries environment, we have to take account of a large variability in the [fish] resources [...] the uncertainty in the state of the stock, variability in recruitment, measurement error, uncertainty in future assessments, future management action, so it’s a complicated process with a lot of different uncertainties. That means to model it realistically you have to have a stochastic process, you can’t simply do it as a deterministic issue. That allows you to have stochastic forecast of the state of the stock on the different assumptions. On top of that you need to have a layer which models the response of the fleet to the catches and the state of the stock [...] and then on top of that you need to have an economic model which represents the different costs and benefits of the behaviour of the fleet as it responds to the state of the stock and to the regulations that are imposed. The whole thing should be a stochastic process [and], in my view, modelled consistently in one package (DG-FISH conservation unit representative-b, personal communication, March 2007).

Meanwhile, in the economic unit, the fisheries economists feared that such a modelling approach, in which biology and economy interact with each other, could prove to be too complicated. They considered the availability of good data to be the most important primary challenge and could do with a simpler model:

Like in everything else you have a model, it's not any better than the data that feeds into it [...] I know what it's like to, when you make a complex model and you try to account for everything, then it just becomes a huge model that only one person understands [...] I do think that it's difficult to find a, say, an exact science that's going to give you an absolute figure that's correct, especially when it comes to long-term simulations. So at least from my perspective, when it comes to this whole modelling exercise [with the flatfish], what I would like to see is spending a lot of time on making sure that the data we have is the correct data and it reflects what the fishery and the fleets are doing and then using some kind of straightforward modelling that we can really all see what's happening and what the assumptions are (DG-FISH economic unit representative, personal communication, February 2007).

This view was coherent with another key concern for the economic unit that was responsible for implementing the IIAs at DG-FISH and that was the need to devise modelling tools valid for the flatfish fishery but generic enough to be applied in future IIAs for other fisheries: “So even if we come out with a good model for the flatfish case, can we really transpose that and use it for the northern hake [fishery]?” (DG-FISH economic unit representative, personal communication, February 2007) In other words, it was expected that the modelling designed for the IIA of the flatfish management plan should have a wide range of application.

In addition to the question of the modelling approach that should be employed in the IIA exercise, there was the issue of who should be invited to the STECF expert group in charge of the IIA. According to the rules, two of the experts had to be formal members of STECF, while the rest should be invited by virtue of their expertise and not as representatives of their country or their employers. As a result, there was a majority of Dutch biologists and economists attending the STECF working group,

given that the Dutch held the largest amount of data and had the most researchers working on this fishery. There were also some Danish economists to present salient economic modelling which happened to have been developed in Denmark. Finally, there were in addition a few social scientists in the working group but their presence remained peripheral. They had been formally invited as qualified experts for the first time in STECF but their particular contribution had yet to be defined and understood<sup>169</sup> (social scientist, personal communication, March 2007). Notably, stakeholders from the NSRAC were also invited to the meeting. Indeed, this was one of the very first occasions that STECF issued a formal invitation to stakeholders to join a working group as observers – with the idea of making the advisory process more transparent and facilitating the exchange of information between scientists and stakeholders (DG-FISH, 2007b).

Another important aspect of the IIA exercise was the functioning of STECF itself. As an advisory scientific body to DG-FISH, STECF was fully in the service of DG-FISH, who routinely set the ‘Terms of Reference’ of STECF working groups – i.e. the formal questions posed to the scientists. Policy-makers often sat in on STECF meetings, actively taking part<sup>170</sup> (social scientist, personal communication, March 2007). For instance, in the case under study, officials from DG-FISH opened the first workshop by explaining the rationale of the plan and framing the tasks to be accomplished (social scientist, personal communication, March 2007).

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<sup>169</sup> Indeed, lacking actual data to work with, they simply examined what kind of information they would need to collect beforehand in future exercises (social scientist, personal communication, March 2007).

<sup>170</sup> In a detailed account of how the science-policy boundary was practised in STECF meetings at the time of research, Delaney and Hastie show that,

[T]hese meetings are driven to a far greater extent by the needs of managers for usable advice. The scientists in STECF are cognisant of what would be “helpful” for the managers to know, while in ICES they are more cautious of providing quantitative assessments in which they have little confidence — they do not want to “do the numbers” so as to endorse a course of action which they are not confident in (2009:676, emphasis in the original).

Meanwhile, in the ICES working groups the biologists barely have the managers in mind as the readers of their work (Wilson and Hegland, 2005).

The ‘Terms of Reference’ for the impact assessment exercise were established on the following grounds: “The first and simplest question is should we do this?” (DG-FISH conservation unit representative-b, personal communication, March 2007) In other words, was it better to apply the HCR in the long-term management plan or to ‘do nothing’ and continue with the *status quo*. This question had to be tackled from both a biological and a socio-economic angle.

In what – in hindsight – proved to be a significant choice, the convenors in the first workshop<sup>171</sup> resolved to form two distinct sub-groups within the working group, one for the biologists and another for the economists<sup>172</sup>, reconvening in a few plenary sessions. The rationale behind this measure was the following:

They were answering different questions and you don’t want to discuss an economic model for instance in the same room with the biologists, they were using different tools and different data and it would have been a rather noisy room. The interaction between the different subgroups was managed [...] on the basis of practicality, subgroups worked separately because it was practical [...] and the economic impacts flowed from the biological impacts (convenor-a, personal communication, March 2007).

In this, the convenors were reproducing the traditional division of labour between fisheries biologists and economists described above. As had been the case to date, biological and economic assessments were conceived in a linear fashion, with annual cost-benefit analyses conducted at STECF adding an extra ‘cap’ on the ICES yearly stock assessment projections. As will become clear in the following, bio-economic modelling for the assessment of long-term impacts would have to learn to take into account much more intertwined dynamics of the biology and the economy of the fishery, which would demand closer and systematic exchange between biologists and economists.

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<sup>171</sup> Formally known as SCEGA-SGRST-06-05 at STECF, as mentioned above.

<sup>172</sup> This seemed to be more or less the procedure at STECF (Delaney and Hastie, 2009).

In this respect, the decision to form separate groups offered little opportunity for any new kind of interaction between the biologists and the economists to flourish. Moreover, the time pressure in the working group meant that it was even more unlikely that progress would be made along these lines, as one of the participants observed: “I think most people are, they’re relatively open to trying to work together, I think, they just don’t necessarily know how. Especially in STECF, when you have so little time to do so much work [...] I think, it’s just very difficult for them” (social scientist, personal communication, March 2007). In fact, business as usual in STECF did not allow for much preparatory work before the actual meetings. While the scientists received an allowance for the actual time spent in the working group, none of the preliminary work before the meetings was covered by STECF (convenor-b, personal communication, March 2007). Furthermore, in the case under study, the experts were called in at short notice. Accordingly, a considerable part of the work on the modelling had to be done *in situ* during the first workshop, taking valuable time from the exploration of the soundness of the long-term plan. As put by one of the participants,

It was just not possible to do all these things in such brief notice [and] I think to do this in a proper way, the modelling work has to be done before the meeting and then you could have a workshop to discuss the results and try other [policy] options and so on (LEI economist-a, personal communication, March 2007).

Having considered the practical organisation of the first workshop of the IIA, I will now address the modelling mobilised in this meeting. I shall do this in a new subsection.

#### **5.4.2 Mobilising economic and biological modelling for the integrated impact assessment**

The first workshop of the IIA opened in the early autumn of 2006 with a four-day meeting in Brussels at European Commission premises. On the first day already the



STECF working group split up into the economic and the biological subgroups. Two different economic modelling approaches were introduced in the economic subgroup.

One of the approaches considered by the economic subgroup was the EIAA model, standing for ‘Economic Interpretation of the ACFM Advice’. Created in 1998 using spread-sheets in Excel, the EIAA model had been used to assess the economic consequences of the annual TAC proposals for EU fisheries conducted by ICES. In brief, it looked at how many fish it was permissible to land in all the EU ports and ran calculations over the profitability for all the various fleets operating at those fisheries, also taking into account estimated variations in market prices. The model had been especially designed to calculate the impact of changes in any quota on all EU fleets governed by such a system. As such, it was intended more to spot broad economic trends across the EU than the precise details of individual fisheries. So, for instance, in the long-term management plan for the flatfish, the HCR had introduced an additional limitation to the fishing effort of the Dutch long beam-trawlers, namely days-at-sea, and the EIAA model had to be adjusted to cope with this new element affecting the profitability of the most important fleet in the flatfish fishery. This was a new context of application, and one that proved to be complex. Effort limitation had to be incorporated into the EIAA model, but this was only possible under the assumption of non-interference with the full exploitation of the entire quotas. In practical terms, this meant that effort days were assumed to prevent the fishermen from exceeding their quotas but not from reaching them – something that might only happen in the case of low fish stocks. In addition, handling a long timeframe was a struggle, since “the original EIAA model was only designed for one-year prognosis so in this exercise we had to repeat the Excel sheet ten times so that each year feeds into the next” (EIAA economist-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

The second approach considered by the economic subgroup was the LEI model – named after the eponymous research institute – which was designed especially for

the sole and plaice mixed fishery in the North Sea. The LEI model was an additional module to the IMARES model<sup>173</sup> introduced to calculate costs and earnings – making it an all-round MSE model. Compared to the EU-wide scale of the EIAA model, the LEI model's scope was rather local; when representing the flatfish fishery, it looked exclusively at the dominant fleet, the Dutch large beam-trawlers<sup>174</sup>, and assumed that this fleet targeted the more valuable sole, plaice being merely a by-catch product in the model. Meanwhile, of the two control mechanisms, TACs and days-at-sea, fishing effort was assumed to be the more important in the LEI model. The reason was simple, according to the LEI modellers. In the mixed fishery, fishermen found it extremely difficult towards the end of the year to match their catch composition – i.e. the species that they find in their nets – with the remaining quota available for each species. In order to continue fishing when they had exhausted one of their quotas, fishermen had traditionally been allowed to discard the excess of that species back into the sea. In other words, the exhaustion of one quota – normally plaice – did not have to constrain the fishing of the other – usually sole – which was the assumption of the EIAA model, as I will explain further below. Hence, the key assumption in the LEI model was that it was the fishermen with their discards, and not the Commission with its quotas, who truly governed the fishery (LEI economist-a, personal communication, March 2007).

Being so disparate, it is easy to understand why there was a clash of sorts between the EIAA and LEI models as they were jointly mobilised in the first workshop. Yet, why were there two economic models in the first place? The answer is linked to the fact that the IIA was a pilot exercise. The original idea of the DG-FISH economic unit was to try to adapt the EIAA model already in regular use at STECF:

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<sup>173</sup> Named after the Dutch marine research institute, this biological model was discussed in the biological subgroup.

<sup>174</sup> Unlike the EIAA model, this model did not consider other fleet segments and 'flags', such as Danish, Belgium, British and even German vessels, which were also involved in the fishery; the assumption was that the dynamics of fishery could be sufficiently explained by focusing just on the Dutch fleet of long beam-trawlers, given that this was the most important one from every respect. Indeed, days-at-sea limitations applied to this fleet only.

We adopted the EIAA approach in flatfish because it was the only tested model by the STECF that had been applied in the past and this is why we chose it for this exercise. We [saw] that it wasn't the ideal model and this is why it had to be [adjusted during the meeting] but it was a starting point and the reason why it was adopted is because we know the assumptions, we know that it has pros and cons [...] And the time [available for the exercise], [the] time limit [was] of course a limiting factor (DG-FISH economic unit representative, personal communication, February 2007).

Therefore, the well-established EIAA model was meant to 'travel' from an already institutionalised context of application to a new one and, although this would not be very sound, the general belief was that it could be done quickly, which was of the utmost importance given the time pressure. On the other hand, the Dutch economists from LEI and the Dutch biologists from IMARES had already managed some time before the meeting to improve the IMARES model<sup>175</sup> with an economic add-on to calculate costs and earnings, and they decided to present this development at the first workshop (DG-FISH economic unit representative, personal communication, February 2007). As follows, the LEI model already travelled to Brussels as an integrated bio-economic set piece, while the EIAA model had to be integrated with IMARES *in situ* during the first workshop.

Once the coupling of EIAA and IMARES was in place, the economists were ready to run both models to see what would happen. The outcome was a staggering divergence between the results of the models (European Commission, 2006b). While the output of the LEI model suggested that the management plan for the Dutch long beam-trawlers was a good idea, the EIAA model indicated that implementing the HCR would be a very poor deal for the Dutch long beam-trawlers<sup>176</sup>. Indeed, the

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<sup>175</sup> Working together as well as with other partners in the project called 'Operational Evaluation Tools for Fisheries Management Options (EFIMAS)', funded under the 6<sup>th</sup> Framework Programme of the European Commission.

<sup>176</sup> The EIAA model anticipated that the expected biomass increase with the implementation of the long-term management plan proposed was not high enough to counterbalance the reduction in effort.

outputs were strikingly opposed, to the relative surprise of all experts in the economic sub-group. This consequently led to a search for explanations within the economic subgroup, focusing particularly on the difficulties experienced in adapting the EIAA model to the new long-term context of application (LEI economist-a, personal communication, March 2007).

However, and this is central, towards the end of the first workshop the EIAA economists wondered whether the actual problem was not in fact inside the IMARES model. In this sense, they decided to address the biologists in the last plenary session. The EIAA economists made an observation to the biologists in relation to a key assumption in the IMARES model, namely the linear relationship in mathematical terms between fishing effort and fishing mortality, which they could not agree with:

The biologists said this is our assumption and you economists have to tell us why should it be different if you want to change it, what is a better assumption and the [EIAA] economists on the other side said well, why should we take the assumption of the biologists, why should the biologists set up what this relationship is. This is our assumption [meaning they also have their own] and why are you biologists using a different one? (convenor-a, personal communication, March 2007).

The definition of the relationship between fishing mortality and fishing effort was central to the implementation of the HCR. The text of the proposed HCR stated that fishing effort would be cut in the same proportion as the reduction needed in fishing mortality (European Commission, 2005b). This was decided on the assumption of a

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Therefore, a negative economic impact was predicted for the Dutch beam-trawlers above 24 metres, with a reduction in net present value of gross cash flow from 278 million euros with the current regime to 210 million euros with the proposed long-term management plan without a discount rate. Meanwhile, the LEI model predicted a larger reduction in effort than the EIAA model, and therefore higher reductions in costs. Furthermore, landings – and thus revenue – were estimated higher than in the case of the EIAA model. In such context, the LEI model anticipated that, for the Dutch long beam-trawlers, the long-term management plan would have a positive impact on the level of gross cash flow compared the baseline scenario – from 260 million euros to 312 million euros (European Commission, 2006b).

linear relationship between fishing effort and fishing mortality, such that acting on the former was believed to translate proportionately to the latter. Notably, the assumption was a working simplification in fisheries biology that was introduced into the HCR by biologists working as officials in DG-FISH (DG-FISH conservation unit representative-b, personal communication, March 2007).

Questioning the nature of this relationship pushed it to centre stage on the final day of the first workshop. Notably, the discussion stimulated real interaction between the EIAA economists and the IMARES biologists, as follows:

The meeting was for a week [...] and my model was run through different scenarios and to supply those economic guys with data, which they processed and drew conclusions. So that was the process starting at day one and at the last day, yeah, this problem of the difference between the [economic models] model came out [...] [And] okay, well, you can doubt about any assumption I make and or we make; [it's] a very simple assumption, the most simple one you can come up with, and I don't see why you couldn't make that assumption for this stock from a biological point of view [...] But if you have another basic idea about this relationship, no problem. But as long as you don't make it too much complicated because that's what I fear, that you create complicated things in your model, which you cannot recognise back after you have run your model (IMARES biologist-b, personal communication, February 2007).

So far I have introduced the divergence between the two economic approaches mobilised in the first workshop and, subsequently, how in the search for possible explanations the biologists' linear relationship between fishing mortality and fishing effort became contested. As I shall describe in the next subsection, the discussion opened up the linear division of labour between fisheries biologists and fisheries economists.

#### **5.4.3 The linear division of labour between biologists and economists comes under pressure**

It is important to understand the challenge that the questioning of the relationship between fishing effort and fishing mortality implied for the linear division of labour between biologists and economists.

This relationship was indeed an issue as old as fishery science (IMARES biologist-c, personal communication, January 2007). Out on the sea, the relationship was a delicate one, sensitive to both the stock of fish in the sea and the behaviour of the fishermen – e.g. choice of gear, of fishing grounds, of target species. Yet, for the production of stock assessments, it was necessary to assume that fishing effort had a constant annual effect upon the fish population – namely by producing a fishing mortality rate<sup>177</sup>, which was characterised as the volume of catches per average stock size over the year (IMARES biologist-c, personal communication, January 2007). The introduction of fishing effort controls, as in the case of the North Sea flatfish fishery, brought this assumption back under the microscope:

Now the [biologists] are being asked to calculate how many fishing days [that is, fishing effort] you need to produce [a given] fishing mortality [...] [But] you can have a very high fishing mortality [or] almost no fishing mortality with the same [fishing] effort depending on the behaviour of the fisherman and his choice of which fishing grounds and the time of the year. It's very difficult to predict what associated fishing mortality [there] is with a certain effort. So if DG-FISH asks, 'scientists, give us the number of days needed for that fishing mortality', it's almost an impossible task (IMARES biologist-d, personal communication, January 2007).

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<sup>177</sup> ICES biologists were used to expressing fishing mortality ('F') as a linear function of fishing effort ('f') on the basis of the efficiency of fishing effort – the catchability 'q' – as a time constant during the year (IMARES biologist-c, personal communication, January 2007).

In this light, the assumption of a linear relationship was the established response from biologists, that is, an ‘exemplary solution’ (Kuhn, 1970[1962]), which could work as long as the time-frame assumed was more or less short:

I think it is a fair relationship to take [it as] one to one in the short-term. In the medium-term you could run into problems because fishing mortality is not only a function of the fishing effort [...] So, of course, there is more behind this relationship but in the short term it’s fair, and in our medium-term simulations we assume simply that there is a one to one relationship although [...] fishing boats are becoming more and more effective year after year (STECF biologist<sup>178</sup>, personal communication, April 2007).

When it came to the evaluation of long-term plans such as the North Sea flatfish plan, this kind of assumption was thus far from robust. The biologists simply lived with it, to the point that some openly acknowledged that the challenge to understand and deal with this relationship went beyond investing in more research:

It is really tricky because it has been always the default assumption [...] that you have this linear relationship. But all the empirical analysis [...] you just plot this effort and in the best [case] you may have relationship but [...] very often you have just a kind of big cloud of points with no clear trend, so I think that’s where we stand now. So we know that this relationship is not so clear that we would like it to be [...] you have so many things that play a role [...] this is a very important scientific question which I think we are far from having solved yet and I’m not even sure we ever do because we will never have much better data [than what] we have now [...] [so] I’m not sure how much we can solve this issue in the future (DIFRES biologist, personal communication, March 2007).

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<sup>178</sup> As an advisory body to DG-FISH, STECF formally enrolls biologists as well as economists. This expert took part in the first meeting of the IIA exercise in his capacity of formal member of STECF – fulfilling the internal requirement of having STECF members together with invited experts at each STECF working group.

Faced with no better answers, the biologists continued to use their exemplar in order to move forward with the long-term modelling needed to assess the new management plan. By contrast, the fisheries economists believed that the behaviour of the fishermen needed deeper consideration than that allowed by the biologists when they put forward a linear relationship:

[T]he biologists say so if it's not proportional, you tell us what we should take then [...] That is of course very complicated. And this is exactly the field that I told you before that economists have to improve their contribution [...] If you really believe that economic decisions matter then sometime it should be possible that, as a group of economists, you say that this relation between fishing mortality and effort is not proportional but it could be in this and this way (LEI economist-a, personal communication, March 2007).

In fact the fisheries economists also had their own exemplary solution to characterise this relationship. In their view, it should be non-linear, since intensifying the fishing effort would in time only lead to a marginal increase in fishing mortality. For the analytical expression of this non-linearity, the economists had borrowed from the neo-classical 'Cobb-Douglas' production function in macroeconomics. If economic production could be expressed as a function of labour and capital then the yield of a fish stock could also be seen as a function of fishing effort and spawning stock biomass<sup>179</sup>. Yet the basic problem for the fisheries economists was how to

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<sup>179</sup> For the EIAA economists a linear relationship was just too simple from the outset; both the density of the fish stocks and the capacity and fishing gear of the fishermen count against it. If the stock doubles its size, the fishermen are not so efficient as to catch twice the amount of fish. As it happened, they had a different equation for fishing effort, based on the neoclassical Cobb-Douglas production function in macroeconomics. Charles Cobb and Paul Douglas originally published it in 1928 (Cobb and Douglas, 1928) as a theory of economic production. It holds that economic production is directly dependent on labour and capital as inputs; meanwhile, technological change is an exogenous variable that affects both the inputs. To the extent that technological progress introduces change in the labour and the capital inputs, the influence of those variations in the output is indicated by two parameters, the so-called output elasticities. Translating this into fisheries economics, the catch (output) can be expressed as a function of the effort and the size of the stock (inputs), with the influence of technological progress in both variables captured by the two output elasticities:

$$h(E, W) = q E^{\alpha} W^{\beta} \quad (\text{see for instance Eide et al., 2003:83})$$



parameterise the function in such a different context as fisheries. Indeed, they could not pin down the non-linearity for each specific fishery across Europe (LEI economist-a, personal communication, March 2007). The economists could at best distinguish between demersal and pelagic fisheries. This is why in the case of the flatfish fishery the EIAA modellers had to use parameter values that applied widely to all EU demersal fisheries. Hence, while fisheries economists could claim that they had an alternative exemplary solution, they could not escape from making broad generalisations, as the biologists.

Confronting the two exemplars at the end of the first workshop raised some eyebrows among both the economists and the biologists<sup>180</sup>. As it happened, this was an atypical situation for the biologists, used to the linear division of labour where the economists and biologists addressed different questions: “I think it was the first time it became so clear that different experts will come with different answers because [...] all the science that we produce is seen from our own expert eyes” (DIFRES biologist<sup>181</sup>, personal communication, March 2007). In effect, the linear division of

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Where ‘h’ stands for catch, ‘E’ stands for fishing effort; ‘W’ is the stock size; ‘q’ is the catchability; finally, ‘ $\alpha$ ’ and ‘ $\beta$ ’ are the so-called effort-output and stock-output elasticity parameters of the production function. In the EIAA model, it is assumed that both the catches and the landings are the same, so the effort-output elasticity reflects the increase in landings for a 1% increase of fishing effort. The default value in the EIAA model is  $\alpha = 1$ , implying that an increase in effort increases landings in the same proportion for a given stock size (W). On the other hand, the relevance of the abundance of fish in the level of landings is regulated by the  $\beta$  parameter, which indicates the increase in the landings for a 1% increase of stock biomass. This parameter ranges between zero and one and the default value in the EIAA model for demersal species is 0.6 (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). This is the same as saying that 1% more of plaice and sole in the sea accounts for 0.6% increase of the catch on deck no matter what additional effort is applied to kill that 1% extra. It follows that the relationship between fishing effort and fishing mortality – defined as the ratio between catch and stock size – is non-linear; linearity would demand the  $\beta$  parameter to be equal to 1.

<sup>180</sup> The decision to make fishing mortality and effort linearly dependent for all species was made by the biologists and they never asked the economists or anybody else (LEI economist-a, personal communication, March 2007).

<sup>181</sup> Some ICES biologists affiliated to DIFRES followed the developments of the IIA exercise from outside the STECF working group. When asked if they should have been included on the basis of their work with the flatfish their answer was the following:

We have the problem that ICES and the STECF are making more and more subgroups and every year you have more subgroups and we are not so many people that can participate to

labour between biologists and economists was no longer viable given the new challenges introduced by the regulatory reforms and the need for the production of interdisciplinary knowledge.

Yet the biologists regarded this as a considerable task and remained generally sceptical regarding the feasibility of producing interdisciplinary knowledge and, in consequence, a different division of labour:

I find it's very good to get this interdisciplinary work but it's very difficult as well. Very difficult to get common concepts and common ideas about what is driving what and how do you parameterise the model [...] I would very much prefer to have simulation models of the biological sort and use the outcome of those models as input for the economic models but keep the two things strictly separate. Because they're in different fields, have very different basic assumptions and in both there is a lot of uncertainty (IMARES biologist-e, personal communication, March 2007).

In contrast, the economists believed that the integration of biology and economy for IIAs demanded a revolutionary step forward: "If you really want to do integrated science it looks like you have to develop a new paradigm because otherwise biologists will be biologists and economists will be economists" (STECF economist, personal communication, March 2007). Of course, the economists argue, such change was not going to take place overnight but, as the example of the relationship between fishing mortality and effort showed, they were ready for a new role:

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these things. So I think, at the end, you know, it's also a matter of priority [...] when you want to have an answer about the flatfish, then the Dutch will answer and then, if you want to have an answer about cod then the Scottish will answer primary [...] I think Denmark is much more diverse in terms of species, and that's the same for our research, which makes that, you know, in Holland everybody almost works on sole and plaice so you have a huge pool of experts but I think in DIFRES [the Danish fisheries research institute] we have to cover the Baltic Sea and a bit of the North Sea [...] which makes that, in the end, I think the pool of species is much bigger than the pool of persons [...] I think, that's one of the reasons why what we do actually is not so strongly accounted for, because when you say flatfish, you say IMARES [...] Flatfish is primarily a Dutch business (DIFRES biologist, personal communication, March 2007).

In my view, where economists could add some information is [over] the uncertainty of the biological models [...] If you look at the fisheries debate between EU managers and the people of the fleets, the basic element in discussion is actually that the managers say you should reduce fishing effort and things will improve in a few years' time. The people of the fleets just don't believe that. If this improvement of stocks would be guaranteed to them, I think then it would be quite easy to change the whole fisheries scene in Europe. But the people of the fleets, the practical fishermen just do not believe that if they reduce a capacity that the situation of the stocks will improve. They say we have done already so much, we have cut several vessels etc. and nothing improves. So the focus is actually, in my view, on the uncertainty of the biologic long run predictions. Do you believe in this or do you not believe in this [improvement]? [...] So I think there is a task for economy in this uncertainty because that is where the real debate is (LEI economist-a, personal communication, March 2007).

In this section, I have shown how the economic models turned into objects of inquiry in their own right – i.e. not just as instruments to conduct the IIA and examine policy options. Furthermore, the modelling process was as much a technical challenge as an organisational one for the experts involved in the IIA exercise. The problems with the cost and benefit calculations were significantly related to the traditional linear division of labour between the biologists and the economists. The pressure on this division of labour between biologists and economists that had been sparked and fuelled by disagreement over the linear relationship between fishing effort and fishing mortality continued to mount in a second workshop of the IIA for the flatfish some months later. I will now address the outcomes of this second workshop in a new section.

## **5.5 The second workshop of the integrated impact assessment**

### **5.5.1 The rationale for a second meeting**

The first workshop did not produce convincing evidence of the costs and benefits of implementing the long-term management plan, and some troublesome issues such as the relationship between fishing effort and fishing mortality were left unresolved. In the aftermath of the first workshop, the STECF working group compiled the in-house

proceedings and drafted a report for the following STECF plenary meeting in November 2006. The STECF plenary meeting was attended again by biologists and economists – either in their capacity as formal members of STECF or as invited experts – together with members of DG-FISH and the JRC. One of the items on the agenda was to peer-review the outcomes of the IIA for the North Sea flatfish long-term management plan. And on this issue the STECF plenary expressed overall concern about the uncertainty that emerged from the economic modelling:

A major aim of the meeting was to evaluate the potential economic performance of the major EU fleets if the proposed management plan were to be implemented. The [working] group attempted to describe the potential changes by expressing the results predicted under the management plan relative to a baseline scenario of status quo fishing mortality. Two economic models were used to investigate the economic consequences of the flatfish management plan: 1) the EIAA-model previously used by STECF and 2) the LEI-model [...] STECF had considerable difficulty reconciling the major differences in model results. As a result at present, STECF is unable to determine whether either of the model results represents a plausible outcome in terms of economic performance. STECF is of the opinion that there are a number of issues for which further investigation and clarification is required, before any confidence can be attached to either of the model results. STECF is aware that the analysis was made under severe time pressure and believes that the working group should be given the opportunity to revisit some of their calculations (European Commission, 2006c:76).

The report drew attention to the scrutiny of the modelling that had taken place during the plenary session<sup>182</sup>, where it had turned into the actual object of inquiry – instead of an instrument to inquire into the impact of the long-term management plan. This focus on the modelling was expected to continue in a second workshop: “STECF suggests that a meeting of no less than 5 days should be convened [...] STECF

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<sup>182</sup> Also confirmed in interview with some of the attendees, as follows: “[At the plenary there] was a whole debate on the models used and not on the way of thinking [with] the models and what we can do with the results of the models” (STECF economist, personal communication, March 2007).

recommends that if possible, for this evaluation, an integrated bio-economic full-feedback simulation model be used” (European Commission, 2006c:77).

Clearly, by sending the experts back to the drawing board, STECF was signalling that the linear ‘piping’ of the EIAA model and the IMARES model was not the way forward. The EIAA model used the biological data as it came out of IMARES model, but the problem observed was the lack of feedback to the biological calculations, as follows:

There was a whole debate on there’s no loop, there’s no linkage between the year on year development in the biological model [IMARES] and in the economic model [EIAA]. Because if you run the EIAA model, basically it [should go] like this: the biological model says you can catch so much, and then the economic model says, yes, but that takes too much effort, too low profit so we will only catch this, then the biological model has to put more as a standing stock into the model because less is being caught (STECF economist, personal communication, March 2007).

Therefore, for the second meeting the STECF plenary asked the modellers in the working group not only to spot possible technical issues in the modelling but also to integrate the EIAA and IMARES models as much as possible – anything in this direction would be considered progress (STECF economist, personal communication, March 2007). Along the same lines, in the eyes of DG-FISH, stabilising the methodology to conduct IIAs was now the priority, since the first workshop had failed to pass the most elementary stage:

The process didn’t get that far really, it was still working on the basic methodology and asking basic questions about data [...] the first stage is to test and develop the methodology [...] we haven’t got past that stage yet and once you’ve got the scientifically workable method, then you can start looking at the questions: well, if the choice is this or that, [or] we don’t like that very much, can we improve it, can we do something else, is there an intermediate solution [...] You have a lot of things you can explore but first you have to get the methodology sorted out (DG-FISH

conservation unit representative-b, personal communication, March 2007).

As follows, officials at DG-FISH aligned themselves with the STECF plenary in advocating more research to try to unravel the uncertainty linked to the economic models and to improve the IIA methodology via a more robust integration of biological and economic modelling. Yet, in practical terms, this implied abandoning the linear division of labour between economists and biologists. Let us look in the next subsection at how the interaction between the two sets of actors evolved during the second workshop.

### **5.5.2 The organisation of the second workshop**

The second workshop<sup>183</sup> had a different flavour from the start compared to the first. The need for a second workshop for the IIA had been strictly established in methodological and technical terms – as reflected in new Terms of Reference<sup>184</sup>. That is to say that the policy relevance of the first meeting had faded into the background for the second meeting<sup>185</sup>, as I will explain in Chapter 6.

The methodological focus also came along with a renewed mandate to find the modelling approach that would underpin the institutionalisation of IIAs – initially for the flatfish fishery and then for any other EU fishery governed by an HCR. By calling for ‘integrated bio-economic full-feedback simulation’ STECF demanded a dynamic model that would take into account how variation in the fishermen’s behaviour in reaction to changes in the economy of the fleet – i.e. in its costs and

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<sup>183</sup> Formally known as SCEGA-SGRST-07-01 at STECF.

<sup>184</sup> What DG-FISH needed at this point was a report (convenor-c, observation, March 2007). This had an echo in the new Terms of Reference, as follows: “Clarify and describe the links between the biological model and the economic model and the differences between the LEI model and the EIAA model” (European Commission, 2006c:77).

<sup>185</sup> Indeed, the meeting did not take place in Brussels but in Copenhagen, at the headquarters of the Danish ‘Institute for Food and Resource Economics’ – FOI for its acronym in Danish – in the spring of year 2007. It is noteworthy that FOI was, so to speak, the ‘home ground’ of the EIAA model.

earnings structure (fuel prices, for instance) – would influence the biological calculations as well. As paymasters, the DG-FISH conservation unit also restated what counted as ‘good modelling’ along the same lines:

The problem which occurred last autumn is that the economic part was not integrated into the biological part [...] what they did was to make it a stochastic simulation [of the] biological part, take simply the median as if it was deterministic, and then apply the economic model upon the median as though it was deterministic. It’s too simple (DG-FISH conservation unit representative-b, personal communication, March 2007).

A central consequence of this methodological focus for the organisation of the second workshop was that the working group ended up being smaller than the first time. Neither social scientists nor stakeholders attended the second meeting as external observers. The former were not called in since the focus was on fixing the modelling and the latter decided not to attend for similar reasons<sup>186</sup> (convenor-a, personal communication, March 2007). Meanwhile, there were fewer economists and biologists due to difficulties in finding experts available from the pool of people who had taken part in the first meeting – particularly in the case of the biologists. Yet, the convenors were not keen to look beyond the original participants merely to find a few more experts (convenor-c, personal communication, March 2007).

Given the small size of the working group, the attendees considered that subgroups as in the first workshop made little sense. The meeting was thus held entirely in plenary. This favoured a completely different type of interaction with respect to the first meeting and it was not long before the experts expressed how much better it was for their task not to be divided into subgroups (STECF SGECA-SGRST-07-01

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<sup>186</sup> Or, as a NSRAC industry representative put it: “After enquiring we found out that it was a technical meeting to see where the discrepancies between the two economic models came from. We [The NSRAC observers] then decided not to go [because] fixing models wasn’t on top of our list and we didn’t see how we could possibly contribute” (NSRAC industry representative-a, personal communication, March 2007).

meeting in Copenhagen, observation, March 2007). In the event, dispensing with subgroups that could only perpetuate the existing division of labour, as well as reducing the size of the group overall, favoured a different dynamic compared to the first workshop, as I shall describe in the next subsection.

### **5.5.3 Reviewing the models and their context of application**

At the outset of the second meeting, the STECF expert group established some common ground regarding the *modus operandi*:

Let's work in a practical sense, from the evidence we agree both of us [EIAA and LEI modellers] down to where we are not sure and work out a way to agree what we think would happen in real life, first of all, and then discuss how we reflect this in the model, rather than arguing, not arguing, sorry, discussing at the model, let's discuss what we think would happen in real life and what evidence we have to support it [...] I think that we should bear in mind that a model is trying to reflect what we think would happen in the real world and therefore is essential to agree what we think is most likely in the real world and then we have a better understanding of what are our differences (convenor-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

In other words, the experts agreed that it was necessary to look at their models as the extension of their world-views. In this respect, in the second workshop the modellers did not simply proceed as if economic and biological models were black boxes that could simply be plugged into one another. This marked a significant difference with respect to the first workshop, as follows: "A big part of what went wrong [in the first meeting] was that figures were taken and copied [from IMARES model], etc., etc., where we, 'a', don't know where they come from and, 'b', we don't know what they represent" (IMARES biologist-f, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). The experience of the first meeting also helped to improve the preparatory work for the second meeting: "One of the problems with [the first meeting] was that it [came] pretty fast and we should have had an initial meeting where we just discussed how the [EIAA and IMARES] models should interact [...] we didn't think about how they should interact before we were



placed in Brussels” (EIAA economist-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

Clearly, any attempt to fix and integrate the modelling could not succeed without opening the ‘can of worms’ of the division of labour between economists and biologists. The LEI model was governed by the biological assumptions in the IMARES model – thus reifying the biologists’ pre-eminence in the advisory arena – and was not convincing for the EIAA modellers. In their view, the LEI model was just a very simple ‘cap’ to the IMARES model (EIAA economist-b, personal communication, March 2007). Believing that their contribution could be more substantial, the EIAA economists challenged the existing hierarchy in the division of labour by means of the modelling:

What is going on here is the key thing, I think. This is whether you want to have the biological model to drive catches, landings, whatever [...] or it should be the economic model which drives really what’s the landings and effort [...] This is also why it is crucial that these things are discussed before detailed modelling is actually started (EIAA economist-b, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

Notably, in the opinion of the LEI modellers, the EIAA modellers were simply looking in the wrong place:

[The EIAA modellers] made a difference between the biological and economic model and I think the difference is between the single-species fisheries [management] and the mixed fisheries [management] [...] if your quota doesn’t match with the catches then you’ll have discarding (LEI economist-a, personal communication, March 2007).

As in the first workshop, the LEI modellers continued to believe that discarding was the distinctive behaviour of the fishermen in a mixed flatfish fishery governed by an HCR, that is, by TAC and effort limitations. In their view, the EIAA economists

were stuck in a single-species approach, in which the quota ruled the behaviour of the fishermen and there were no discards.

At one point in the workshop when the EIAA modellers unfolded all the core assumptions of the EIAA model to establish a comparison with the LEI model, three major areas of divergence were identified: the calculation of landings<sup>187</sup>, which was a key factor in understanding the economic turnover of the fishing activity; the calculation of the effort (days-at-sea)<sup>188</sup>, which was a key factor in understanding the economic costs; and the calculation of the stock biomass, considered by the EIAA modellers to be a variable for the calculation of fishing effort. I will take a detailed look at the latter since it was this issue in particular that led to the thoroughly opening up of the biologists' IMARES model for the very first time in the IIA

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<sup>187</sup> The EIAA model was bound to the TACs and the quota system that imposed limits on landings of fish. It assumed that nothing more and nothing less than the quotas were caught, fulfilling in this sense the principle of relative stability. The problem with the calculation of landings for the EIAA modellers was that even when fishermen exceeded their quota, the model would disregard the excess catch, creating the so-called 'missing fish' problem. In addition, if the fishermen caught less than the quota, then the EIAA model would count some of the fish still swimming in the sea as landings. In the case of the LEI model, these problems did not exist because it tracked down the catches over the quota that occurred at sea despite the principle of 'relative stability'. In order to do so, it used a different assumption for fishermen's behaviour: the existence of significant levels of discarding. Knowledge of the Dutch fishermen's practices was key, if only because it was not easy to figure out the levels of discarding. In this sense, for the Dutch LEI modellers, local information about the behaviour of the Dutch fishermen was available since they enjoyed regular communication with the fishing sector. Indeed, they were able to refer to detailed empirical data from this fishery. For example, they knew that over the last years the fishermen had not been able to fully exploit their valuable sole quotas, sometimes to a large extent (LEI economist-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). All in all, by trying to make the landing calculations comparable, the EIAA and LEI modellers were again confronted with the assumption of whether the fishery was fundamentally governed by the EU-wide principle of relative stability or driven by the much more situated discards – all in all, different world-views confronted each other (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

<sup>188</sup> As in the case of landings, there were two different calculations of effort. For the EIAA model, effort calculation was defined as a function of the stock size (biomass). In this sense, for instance, the amount of effort needed to catch a fixed TAC would be halved if the biomass doubled. On the other hand, the LEI model assumed that the behaviour of the fishermen at sea could be considered more or less independent of the amount of fish biomass; the effort pattern could be seen as constant – thus overlooking the technological improvements, the so called 'technological creep' – and the only way to reduce it was through days-at-sea limitations, decommissioning of vessels or manipulating the gear, to cite the most common policy options. By and large, there were very different views between the LEI modellers and the EIAA modellers regarding the effort calculations (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

exercise (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

As already mentioned, both the EIAA model and the LEI model worked in conjunction with the IMARES model – the difference being that the LEI model had been specifically designed to be coupled with the IMARES model. In the second workshop the experts realised that the EIAA and LEI models used two different calculations for the fish stock size or the so-called ‘spawning stock biomass’ (SSB), which could only be a source of error. It was a striking finding because the EIAA model borrowed the figure straight from the IMARES model and for the EIAA economists there was no reason to believe that it could be different to the one used by the LEI modellers – also taken from the IMARES model. There was a reason for the disparity, nonetheless; as the working-group only then realised, it was related to how knowledge about the fish stocks was produced by the biologists at ICES (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

The IMARES model, so far a black-box for the EIAA modellers<sup>189</sup>, had a complicated internal structure, which combined two strands of ICES biology, so to speak: the study of fish population dynamics and the stock assessment calculations. On one hand, population dynamics modelling predicted the so-called ‘true’ size of the stock with all the existing data from scientific surveys and assumptions about fish recruitment, growth, life span, natural mortality and fishing mortality. On the other hand, another epistemic route to the dynamics of the stock size was possible for ICES biologists by using the catching fleets as a proxy. This was what was done in stock assessment by tracking how well the fleets were doing with landings and what fish sizes and ages reached the markets. After processing all this data for each type of

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<sup>189</sup> Yet, it was built using ‘R’, an ‘open source’ programming language popular among statisticians. The aim of IMARES modellers was to be as transparent as possible, beginning with their fellow biologists (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

stock, ICES biologists came up with the so-called ‘perceived’ assessment of the size of the stock. In theory, both the ‘true’ and the ‘perceived’ stock sizes should be the same but, in practice, known methodological biases in stock assessment tended to result in systematic overestimation of the size of the stock (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

The fact was that stock assessments were used to inform annual management decisions in EU fisheries, which had an impact on the fishermen’s catching opportunities the following year, which in turn had an impact on the stock population dynamics with their fishing. This cycle was repeated year in, year out. Therefore, any modelling that aimed at projected scenarios of the flatfish fishery in 2015 under the conditions established by the management plan ought to be looking not only at things like stock recruitment or fishermen’s behaviour, but also at how well the ICES biologists performed their stock assessment duties. And so IMARES biologists thoughtfully included themselves in their modelling, in their capacity as members of the ICES community, taking into account their systematic tendency to overestimate the stock sizes<sup>190</sup>, indeed another factor that would influence the fishery:

It is actually a crucial thing to what we do because we can evaluate our own misperception of what’s going on and [...] for these models is crucial to understand what the difference is between this perceived and the ‘true’ estimate [of the spawning stock biomass] [...] But that’s basically where the problem boils down to [because] the EIAA model was applied to the biological part without realising what was going on inside (IMARES biologist-f, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

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<sup>190</sup> This bias was not stock specific. It was partly due to the lack of information on discards or underreporting or misreporting of landings, leading to an underestimation of the fishing mortality and, therefore, to an overestimation of the stock size (Reeves and Pastoors, 2007; similar cases have been reported elsewhere outside the ‘ICES area’, see for instance Finlayson (1994) for the Newfoundland cod fishery).

In other words, the problem was the established linear division of labour, the sequential line-up of two independent epistemic frameworks: biological and economic. The EIAA model used the ‘true’ stock size when it should have borrowed the ‘perceived’ figures according to the IMARES modellers. Yet, beyond this technical issue, the IMARES model structure was generally opaque to outsiders, including the EIAA modellers. In other words, the IMARES modellers managed to black-box ‘two biologies’ within their modelling for the whole first meeting and almost the entire second one, proving the biologists’ ascendancy in the linear ordering of the division of labour. At the same time, the fact the IMARES model was finally being scrutinised was a strong sign that the linear division of labour was coming under pressure in the second workshop.

On the evidence presented so far, the additional work undertaken in the second workshop prised open the models beyond the issue of the relationship between fishing mortality and fishing effort and simultaneously called into question the linear division of labour. This was without a doubt a necessary pre-condition for the convergence and integration of the EIAA and LEI modelling approaches pursued by STECF and DG-FISH. However, the more the models were unpacked, the less compatible they, and the worlds they inhabited, appeared to be. Progress towards an integrated bio-economic modelling approach still demanded assimilation of very different world-views, along with the ‘scrapping’ of the established division of labour. And yet the second workshop did offer some first signs of ‘spring’, as I shall illuminate in the next subsection.

#### **5.5.4 The emergence of a ‘trading zone’**

As seen in the previous subsection, during the second workshop the modellers looked deeper into their models and identified shared elements across EIAA and LEI models that offered some access for better integration of biology and economics. They generally found the task difficult, but they did manage to make some progress,

as for instance in the revision of the conception of the relationship between fishing effort and fishing mortality described in the first workshop.

In the second workshop, this relationship came back under the spotlight. As one economist stressed: “This relationship has a huge impact on the economic analysis, whether you have to use more effort to catch a similar amount of fish [per average stock size] or not” (LEI economist-b, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). The view from a biological counterpart followed in similar terms, also detailing the rationale underlying their approach:

The thing is that it has a huge impact on the biological side [...] We just make a simplification [...] We consider fishing mortality and fishing effort being equal, ‘a’, because lack of data to tell you otherwise and, ‘b’, because is a good zero-hypothesis [...] In general, we have been very open about the fact that we use this linear relationship to make calculations while in reality, if we look back historically, the [ICES] assessments show a huge variation in the fishing mortality and effort relationship. The main driver for this huge variation in the historical relationship is the fact that this [flatfish fishery] is a mixed fishery that you manage with two TACs. If you have a TAC that is very big whereas the other one is very small you will redirect your fishing effort and also shift this [relationship between fishing mortality and fishing effort] [...] [The fish] stock density plays a role but not as much as the fact of running the mixed fishery with [two] TACs (IMARES biologist-f, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

The biologists, therefore, pointed to the new regulatory framework, with its influence on fishermen’s behaviour in the mixed fishery, as the main reason for a non-linear relationship. Yet, for the biologists the question was which form of non-linearity. It was difficult to uphold an argument for any one in particular (IMARES biologist-f, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). Therefore, they advocated their linear exemplar, which was at least a reasonable and easy-to-handle approximation in their view.

Meanwhile, for the EIAA modellers, a linear relationship was just too simple from the outset. They had a different exemplary solution in their book of tricks, as mentioned earlier. They advocated a non-linear relationship in which the efficiency of the fishing depended on the stock size – if you double the stock, you will not catch twice as much fish (EIAA economist-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). In response to the biologists' lack of support for any particular non-linear function, the EIAA modellers argued the case for the Cobb-Douglas function, using the standard parameterisation that applied to other demersal fisheries in Europe.

As in the first workshop, both the EIAA modellers and the IMARES modellers were confronted with the gulf between their exemplary solutions. Nevertheless, this time the expert group crucially managed to bridge it. It all began by establishing the following criterion: “Actually, the discussion [about this relationship] is on simplicity or accuracy, a classic trade-off we have to make every time we try to model” (convenor-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). As follows, the expert group agreed that the accuracy in the modelling should not to be traded for simplicity regarding a relationship that was so crucial for the long-term foresight:

Convenor-a: We are trying to say to people [outside this working group], okay you can go for simplicity because it is quicker and we can all understand it with less mental effort, but, the choice between simplicity in the approach to this [relationship] or an accurate approach to this [relationship], changes the decision between whether the baseline [*status quo*] or the management plan gives you the best scenario [...] So we can say, that's very complex, but hey, sometimes fisheries are complex and if we go for just the simplicity route you can end up with the wrong answer to your problem. [...] [T]he choice of a simplistic linear relationship or a more complex non-linear relationship can change the answer between the management and the baseline scenario and, therefore, because of the importance of this relationship, we should go for accuracy over simplicity.

LEI economist-b: Uh, you should look into it, because for some of these fisheries it might be most appropriate to use a linear relationship.

Convenor-a: Yes, if linear is accurate.

LEI economist-b: Yes.

Convenor-a: So, I don't say we should prefer complexity over simplicity, but accuracy over simplicity (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

At this point the ground was set and when the EIAA economists then moved on to discuss the particular case for the flatfish fishery, the LEI economists argued that the biologists' linear solution could be an accurate answer:

In the flatfish fishery, the [Dutch] beam-trawlers do not have many options to choose [that is, to employ their fishing effort] – like leave the fishery in the low season to do other things more profitable – so [the relationship between fishing effort and flatfish fishing mortality] should be close to linearity. Maybe for other fleets [with more options to fish than flatfish] is different (LEI economist-b, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

The LEI economists had just brought in a situated argument for the decision on linearity versus non-linearity<sup>191</sup>. Given that the Dutch fleet of long beam-trawlers would be simultaneously restricted by the two mechanisms in the HCR, quotas and days-at-sea, and with no real fishing alternatives from which to make their living apart from sole and plaice, the linear relationship could be reasonably accurate on average according to the LEI modellers. Their point was that in the absence of any significant captures other than sole and plaice, the pattern of fishing would be

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<sup>191</sup> Notably, the EIAA economists welcomed the very fact that there was a discussion: "This is a crucial issue and we should discuss it before we even start modelling. The problem in September [2006] was that we had two models but we didn't discuss the crucial parameters before we started to run the models" (EIAA economist-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).



predictably constant for this fleet segment and, therefore, when at sea, their fishing effort would produce a fishing mortality more or less in the same proportion because they would generally know where to find the flatfish, regardless of the level of scarcity. However, according to the LEI economists, this was just the local idiosyncrasy of the Dutch beam-trawlers and linearity could not be embraced as a rule of thumb in the analysis of any kind of fleet, as the biologists tended to do: “Linearity may be present in some fisheries but not necessarily in general. I mean you can have the linearity [in the model] but [...] there are many good reasons why non-linearity could be present as well. So just assuming linearity would be [a matter of] further investigation” (LEI economist-b, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

It was clear that the LEI modellers played a key role of mediation. This was possible thanks to their rather short social distance to the EIAA economists and the IMARES modellers<sup>192</sup>, as well as their access to the Dutch long beam-trawling fleet (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). By and large, their contribution introduced some flexibility with the context of application of the relationship. It was no longer a dichotomy between linearity and non-linearity at an absolute scale, as if one of these analytical expressions – linear or non-linear – should be finally picked up and standardised for any fishery in the IIA methodology. In other words, a decision on such an important relationship should not be ‘carved in stone’. The suggestion by the LEI modellers was to bracket off the so far rather universal character of the two exemplary solutions. In so doing, the LEI modellers argued that the linear assumption could apply accurately to the local context of Dutch long beam-trawlers, while the non-linearity could remain a better assumption on average for the bigger picture of EU fisheries. As it happened, the working group agreed that experts with local knowledge could best advise on the nature of the relationship between fishing mortality and effort at the local scale of a fishery, or

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<sup>192</sup> It is noteworthy that one of LEI economists had been trained as a biologist.

even at the scale of different fleet segments within a fishery (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

By and large, the disciplinary boundary between economists and biologists had turned into a ‘trading zone’ (Galison, 1996, 1997). As a result of the trading performed, the linear assumption for the Dutch long beam-trawlers was no longer the application of the biologists’ exemplar but an interdisciplinary answer to the situated question of the relationship between fishing mortality and effort on this particular fishery. Notably, consideration of the linear relationship as an accurate approach for the Dutch long beam-trawlers became compatible with its straight character. In other words, as well as being accurate enough for the economists, the linear relationship was usefully simple, as the biologists had been advocating.

The successful trading over the relationship between fishing mortality and fishing effort helped to boost confidence within the STECF expert group by the end of the second workshop: “Integration of the models is possible, now that the links between them are better known” (IMARES biologist-f, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). Still, for the STECF expert group this was not just a question of knowing the links between the biological and economic models but also a question of continuing to reshape the linear division of labour: “In order to achieve proper integration of the models, the experts need to be integrated” (convenor-a, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). Their observation was subsequently reinforced at the STECF Spring Plenary in April 2007 as follows: “[B]io-economic modelling is not a one-way affair” (European Commission, 2007:14).

## **5.6 Conclusions**

The reform of the CFP in 2002 introduced long-term management plans as the new central tool for the governing of EU fisheries and the establishment of the Better

Regulation strategy – also in 2002 – added the IIA to the set of policy instruments. As part of the implementation of an IIA for the first time in DG-FISH, in this chapter I have placed the focus on the arrival of novel (bio-economic) integrated assessment modelling, which offered a space for the constituencies of the fisheries advisory arena – biologists and economists – to revisit their division of labour. I will wrap up the main insights in two sets.

First, (ICES) biologists and (EIAA) economists – actors in the annual production of advice to DG-FISH – pursued the control of a new advisory arena for long-term management plans by means of their modelling. The process led to social learning and redistribution of authority, which the framework of co-production helps to inform. The chapter offers empirical evidence of the twofold co-productionist process with its account of the ‘two linearities’; that is, on the one hand, the emergence of interdisciplinary knowledge about the linear mathematical relationship between fishing effort and fishing mortality for the flatfish fishery and, on the other hand, the shifting from the linear-sequential – or ‘mechanistic’ (Burns and Stalker, 1994[1961]) – division of labour between biologists and economists to a more ‘organic’ (Burns and Stalker, 1994[1961]) one. As much as trading on the character of the relationship between fishing mortality and fishing effort, they were also trading or mutually adjusting who-had-authority-when in the scientific advisory arena.

The second set of conclusions concerns the shifting role of the computer modelling. In the first workshop of the IIA exercise the modelling led to dissent, beginning with different cost-earning futures and ending with some contestation between economists and biologists over the relationship between fishing effort and fishing mortality. In the second workshop the modelling turned into the object of inquiry as opposed to a means to aid policy-making. There was more contestation at the start but, by the end of this second workshop, the modelling illuminated a ‘trading zone’ (Galison, 1997, 1996) where actors made sense of each other. By and large, the modelling emerged

as a space for some still modest degree of dismantling of the linear division of labour between biologists and economists – giving way to ‘green shoots’ of a more interdisciplinary collaboration, at least in the case of IIA exercises. All in all, the integrated assessment modelling became constitutive of the dynamics of change around the division of labour between economists and biologists in the scientific advisory arena – i.e. of the dynamics of the building of a new sociotechnical framing for EU fisheries management.



## **6 Building a new framing for the North Sea flatfish fishery: The introduction of integrated impact assessments**

### **6.1 Introduction**

Following from Chapter 5, this chapter will focus on the second of element of what I call the troika of instruments – consisting of computer modelling, integrated impact assessments and harvest control rules – for the depoliticisation of decision-making in EU fisheries management. Drawing on the case study of the North Sea flatfish fishery, I will continue to explore the dynamics of social learning and re-assignment of authority associated with the building of a new sociotechnical framing for managing EU fisheries, but this time at the level of the IIA exercise as a whole.

While the modellers focused the attention of the previous chapter, here it will be the turn of the stakeholders. I will throw the spotlight on the biological subgroup in the first workshop of the IIA exercise. Mainly composed of biologists and NSRAC fishing industry representatives<sup>193</sup>, the biological subgroup became a space in which fisheries management choices were discussed with the help of the IMARES model. As I shall show, the NSRAC industry stakeholders engaged with the biological model as an empirical ally that furnished them with authority. Instead of challenging the biological assumptions and scrutinising the uncertainty – as they often did with the annual stock assessments – they used the IMARES model to legitimise an alternative policy scenario. This was even more remarkable from the point of view that the IMARES model was somehow work-in-progress, as I shall explain. All in all, I will show how the NSRAC industry stakeholders domesticated the IIA exercise, blurring the distinction between the scientific and the political arenas.

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<sup>193</sup> In the NSRAC the fishing industry outnumbered any other social group and exercised control over the organisation.

The structure of the chapter is as follows. In §6.2 I will provide a prehistory of the IIA that will serve to contextualise how the NSRAC fishing industry stakeholders approached the biological modelling in the IIA exercise. First, I will address the production of advice by the NSRAC ahead of the publication of the long-term management plan proposal for the flatfish fishery by DG-FISH in January 2006. Second, I will consider the interaction between the NSRAC industry stakeholders and the scientists at IMARES in order to evaluate the soundness of the advice to the Commission produced by the NSRAC in anticipation of the flatfish management plan proposal. Third, I will address the publication of the Commission's proposal and describe why the NSRAC and the former Dutch Ministry of Agriculture, Nature and Food Quality decided to problematise it, a process that finally ended with the launching of the IIA exercise. In §6.3, I will interrupt the chronological account and make a detour to highlight the limitations behind the development of the biological model at IMARES, particularly when it came to representational validation. In §6.4 I will resume the chronological account of the case study and discuss how the NSRAC industry stakeholders approached the modelling in the first meeting of the IIA. In particular, I will look at how their participation was organised and what kind of engagement with the IMARES model emerged given the considerations presented in the previous two subsections. In §6.5 I will address the aftermath of the first workshop of the IIA exercise and how a decision on the long-term management plan was finally reached. I will end the chapter offering some conclusions in §6.6.

## **6.2 *The prehistory of the integrated impact assessment***

### **6.2.1 The initial advice from the NSRAC to the Commission regarding plaice stocks**

As described in Chapter 4, the Regional Advisory Councils were formally introduced after the reform of the CFP in 2002. It took a couple of years before the first of the RACs, the North Sea Regional Advisory Council, saw the light of day, in November 2004. From the earliest days the NSRAC drafted a number of advice reports and

position papers to the Commission. Notably, between the end of 2004 and 2006 much of this policy advice came from its former Flatfish Working Group.

Work in the Flatfish Working Group of the NSRAC began when DG-FISH made a first consultation for advice to design a long-term management plan for the North Sea plaice and sole fisheries in the autumn of 2004. Due to the fact that the request came at short notice while the NSRAC was still in the process of being established, the various constituencies within the NSRAC had to agree very quickly on an initial response, which focused entirely on the plaice stocks (NSRAC, 2004a) since the DG-FISH conservation unit had a much lesser sense of urgency concerning the sole stocks.

Producing unified advice was far from straightforward for the NSRAC. On one hand, the flatfish industry was ready for a medium-term management strategy for the plaice stocks in the North Sea. The Dutch fishing industry in particular was keen to see a 15% reduction of fishing capacity in the 80mm net mesh-size beam-trawler fleet in order to reach a better balance between fishing capacity and flatfish catching opportunities. On the other hand, this offering was considered poor by environmental NGOs and the recreational angling sector within the RAC (NSRAC, 2004a). Yet in the end they adopted a pragmatic attitude and accepted the Dutch flatfish industry's medium-term plan as interim advice to the Commission. In essence, it would always be better than doing nothing (NSRAC environmental NGO stakeholder, personal communication, February 2007). Further, at that point in time the NSRAC did not have the time or the means to produce anything more ambitious and scientifically sound.

Lack of access to a pool of scientific experts was indeed a matter of concern for the NSRAC stakeholders at large, since they felt an acute need for stronger scientific grounding when providing advice to DG-FISH:



The NSRAC is not (yet) equipped with an appropriate biological and economic knowledge network to facilitate the discussions leading to its advice. In the case of this advice on plaice management, the absence of external experts hindered the development of advice [...] For the same reason, an assessment or quantification of the biological and economic effects of the proposals made could not be carried out, but is considered necessary (NSRAC, 2004a:1).

Accordingly, the NSRAC industry stakeholders were eager to have scientific assessment, not just in biological terms, but also in terms of economic evaluations of management measures. They often used the case of the ‘plaice box’ to illustrate the poor biological impact and serious socio-economic impact of new regulations that had not been properly evaluated beforehand:

[E]x-ante and ex-post evaluation of management measures (including a biological, socio-economic and enforcement assessment) is in line with the principles of good governance [...] While new fisheries management measures can have severe socio-economic consequences on the fleet the supply chain and fishing communities both in the short and long term [...] an economic assessment is generally not included [...] This is an omission which must be addressed (NSRAC, 2004a:3).

On receiving the interim advice from the NSRAC on medium-term measures for the North Sea plaice stocks, the DG-FISH conservation unit remarked that the focus of the final formal advice should be on the long-term and that the goal of DG-FISH was to carry the stocks to a new production regime of high yields; in other words, the DG-FISH conservation unit was aiming for a long-term management plan for plaice and sole rooted in strong political commitment to MSY, following the international agreements of the WSSD 2002 (NSRAC, 2004c).

While welcomed by environmental NGOs (see §4.4), the fishing industry representatives within the NSRAC received the insistence of DG-FISH on MSY with

distress<sup>194</sup>. They claimed that embracing MSY would have significant economic impacts on the fleets and that it should be debated thoroughly beforehand (NSRAC, 2004c). In their view, rather than a long-term plan with MSY ambitions, it would be more sensible to begin with medium-term measures that could bring the North Sea plaice stocks back to precautionary levels over the course of a few years (NSRAC, 2004a, 2005a). Indeed, the NSRAC warned the Commission about the importance of winning the support of the fishermen before defining any long-term strategy (NSRAC, 2005b). On the other side of the table, the Commission reminded the NSRAC that lowering fishing mortality substantially would lead to higher yields in the range of MSY and more stable and sustainable fishing for the fleet according to ICES. Moreover, they argued that many Member States had expressed a strong commitment to MSY (DG-FISH conservation unit representative-b, personal communication, March 2007). Accordingly, any advice by the NSRAC should necessarily focus on how to achieve MSY, a binding objective for DG-FISH (NSRAC, 2005b).

Believing that it could help to strengthen the NSRAC's provisional recommendations by means of modelling, the Dutch fishing industry in the NSRAC brought the interim advice to the LNV, which had the means to commission the Dutch institute for Marine Resources and Ecosystem Studies – IMARES<sup>195</sup> to assess scientifically the consequences of the measures (NSRAC industry representative-a, personal communication, January 2007). Through some *ad hoc* modelling<sup>196</sup>, IMARES modellers showed that a reduction in fishing effort – days at sea – for the 80mm fleet was the most direct way to restore the plaice stocks – as compared to other measures, such as the increase of the mesh size of the fishing nets. An effort reduction of 30%

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<sup>194</sup> The problematisation of an MSY-based management approach for the flatfish fishery within the NSRAC was led in particular by the Dutch fishing industry, fearing that such management objectives would imply reductions of more than 50% in the Dutch beam-trawler fleet (NSRAC industry representative-a, personal communication, January 2007).

<sup>195</sup> Better known by the former acronym of RIVO at that point in time.

<sup>196</sup> That was indeed the embryo of the IMARES model discussed in Chapter 5.

in particular seemed to offer a high probability of letting the plaice stocks recover to precautionary levels within two years, whereas smaller reductions of 20% and 10% would extend the timeframe to three and six years respectively. Yet the degree of uncertainty in these results was high, as openly acknowledged by the IMARES modellers<sup>197</sup> (Grift et al., 2005).

In light of the high uncertainty, the NSRAC industry stakeholders decided to adhere to their initial suggestion of an effort reduction of 15% in the 80mm flatfish fleet<sup>198</sup>. Indeed, it could be an opportunity to study empirically how effectively the decrease of fishing effort translated into a recovery of the spawning stocks' biomass to precautionary levels in the medium-term<sup>199</sup> (NSRAC industry representative-a, personal communication, January 2007). In their view, the 15% reduction was already sufficient for such a trial, and in the end this was the core of the formal NSRAC advice put forward to the Commission, as follows:

[A] multi-annual management plan should be adopted for plaice in the North Sea with an initial target of reaching an [spawning stock biomass at the precautionary] level within 3–5 years with a re-evaluation after 3 years and with the long term aim of exceeding [precautionary level]. The plan should be implemented as of the 1st of January 2006. The management plan is aimed at reducing pressure on juvenile plaice and would comprise structural effort reductions accompanied by stability in the TAC for plaice. The multi-annual plan should be accompanied by a monitoring and evaluation scheme, which would also include the monitoring of social and economic impact [...] To reach the target of the multi-annual management plan, the NSRAC advises a structural effort

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<sup>197</sup> In this sense, the IMARES scientists included the following warning in their report: "It should be noted that the results of the simulations should be interpreted with caution. The exact values are strongly dependent on the parameterisation of the model. Landings and stock biomass trends will be very sensitive for the assumption of the incoming recruitment" (Grift et al., 2005:31).

<sup>198</sup> Notably, other flatfish fleets were excluded, like the 100mm and 120mm mesh size beam-trawlers, which produced a much lesser fishing mortality for the plaice (NSRAC, 2005d).

<sup>199</sup> It is noteworthy that the NSRAC industry stakeholders tended to assume a fully responsive linear relationship between fishing effort and fishing mortality. In their view, reductions of effort equate to reductions in fishing mortality (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

reduction of 15% of enforced licensed capacity limits in the international 80mm flatfish fishery over 2006 and effort to be maintained at the new level for a further two years (NSRAC, 2005a:4).

Notably, the advice was presented to DG-FISH as a consensual achievement of the member organisations of the NSRAC with stakes in the flatfish fishery (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

The advice focused on the plaice stocks but the fishing industry believed that, in the mixed fishery, effort reductions should benefit both plaice and sole (NSRAC, 2005a). Meanwhile, there were no references whatsoever to MSY. In supporting their argument against this objective, the industry representatives in the NSRAC claimed that the implementation of the MSY strategy had not yet been formally discussed at the highest political level. The NSRAC did not want MSY imposed as the long-term target before policy-makers had at least considered other options for defining long-term objectives such as economic sustainability and the stability of the fleet (NSRAC, 2005a).

However, the DG-FISH conservation unit insisted once more that there should be no long-term alternative to MSY. Concern was also expressed at DG-FISH about the lack of strategic ambition in the advice from the NSRAC, with the 15% total reduction in effort clearly regarded as inadequate (NSRAC, 2005c). In addition, DG-FISH criticised that the advice from the NSRAC was again limited to the plaice stocks, disregarding direct considerations for the sole stocks.

Notably, the NSRAC's advice raised questions not only within the Commission but also within the Dutch fisheries ministry. The LNV was particularly concerned about whether a 15% reduction in effort in the 80mm mesh-size Dutch beam-trawler fleet would affect the more profitable sole stocks. As I will describe in the next

subsection, the Dutch government decided to make their own evaluation of the scientific soundness of the formal management advice of the NSRAC.

### **6.2.2 The linear division of labour between NSRAC industry stakeholders and IMARES biologists**

The LNV considered that it was worth exploring scientifically the potential effects of the NSRAC's advice on *both* plaice and sole stocks. For this reason, the LNV commissioned IMARES to evaluate how the NSRAC management plan for plaice would impact on the sole fishery. The mandate was somehow a follow-up of the earlier task commissioned to IMARES by the LNV described in the previous subsection. However, this time the biologists from IMARES confessed to having difficulties with the exercise. They found that the NSRAC proposal could not be dealt with in a scientific manner because of the presence of many loose ends in the formulation of the management advice:

It was very clear from the beginning of the process that the management plan of the NSRAC was not specific enough for a simulation approach. Simulating management plans requires that all eventualities are covered in the plan and that no ambiguities are left. However, in order to lift those ambiguities, a close connection between the different parties would have been required (Poos et al., 2006:14).

By and large, this lack of interaction between the NSRAC and IMARES had become a major problem for the evaluation. Only when the results of the evaluation were ready did a proper meeting take place to present the results to the NSRAC<sup>200</sup>. IMARES scientists explicitly observed that it had been a “very linear process” (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006;

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<sup>200</sup> It has to be said, however, that December 2005, when the evaluation took place, was a very difficult time for the scientists to start an open dialogue with the industry due to the negotiations over the quotas in the Council of Ministers. Indeed this is probably the most stressful period of the year for the fishing industry. Thus, to some extent the difficulties that the IMARES researchers experienced to start a dialogue on how the measures of the NSRAC advice should be interpreted and articulated in the model (IMARES biologist-e, personal communication, March 2007).

see also Pastoors et al., 2007), with very little interaction with the NSRAC as masterminds of the plan.

The consequence of this lack of fluent communication was that the scientists had to make some decisions for which they were not happy to bear responsibility (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). For instance, when the NSRAC advised an effort reduction of 15% of “enforced licensed capacity” (NSRAC, 2005a:4), it allowed some room for interpretation. Vessels may have under-utilised extra capacity – for example, vessels with a very powerful engine – and reducing this capacity might have no effect at all in the *de facto* fishing effort levels applied. IMARES scientists decided alone that the 15% effort reduction should be interpreted for the evaluation as a true reduction in the actual effort – by scrapping vessels, for example.

This evaluation was also a challenge for the IMARES scientists for other reasons. Since the very beginning they had decided to develop a sophisticated MSE modelling approach to evaluate the NSRAC management ideas. However, there was a price to pay for this decision, partly because, when it came to the parameterisation of the new model, the IMARES modellers had to do a lot of “guesstimating”<sup>201</sup> (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006; see also Pastoors et al., 2007). In other words, they simply did not have enough data to parameterise and validate the new model.

When the projections of the IMARES model were presented to the NSRAC they were expressed as probabilities of reaching the precautionary scenarios for plaice and sole in the medium-term – by 2010. In the case of plaice, this varied between 62%

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<sup>201</sup> For instance, they used a survey to study fish migration patterns carried out only in September as if the data were valid for the whole year (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

and 98% depending on the assumptions, while for sole the probability was lower, between 48% and 82%, due to an expected low ecological recruitment<sup>202</sup> in the short-term (Grift et al., 2005). In presenting the results as a range, the IMARES modellers were explicit about the uncertainty behind the results of the modelling (see Pastoors et al., 2007). Across the table, the NSRAC fishing industry representatives were expecting clear signals about the medium-term development of the stock that they could use in the policy process. In other words, they wanted to keep things rather straightforward (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). In this sense, their interpretation of the results of the modelling was that there would be quite a high probability of recovering the plaice stocks to precautionary levels if the NSRAC advice of a 15% reduction was implemented. Therefore, in their view, the NSRAC advice for plaice went in the right direction. Meanwhile, for sole stocks they saw that ecological recruitment failure would make short-term recovery unlikely and suggested that perhaps additional measures other than effort reduction could be implemented (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006).

Notably, having been explicit about the weaknesses in the modelling process at the final meeting with the NSRAC, the IMARES scientists were staggered to find that the NSRAC fishing industry did not contest the outcomes as they usually did with the stock assessment process (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). The only issue on which the NSRAC industry stakeholders were slightly harder to please, according to the IMARES modellers, was the degree of verisimilitude when running the model over a few years in the past. When comparing the outputs of the simulation with the observed figures of the past, the NSRAC industry stakeholders tended to read the results rather closely, that is, disregarding typical error ranges. IMARES scientists running the model

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<sup>202</sup> Defined as the increase of the natural population due to propagation.

backwards observed, for instance, that if annual landings calculations differed by just a few thousands of tons with the data from the past, some NSRAC fishing industry representatives would say that the model was wrong (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). Yet this seemed more circumstantial than a real belief among the NSRAC industry stakeholders that this kind of model could deliver accurate predictions. The NSRAC industry representatives openly acknowledged that while some of the initial assumptions underlying the simulations could have benefited from more discussion, there were no major issues at stake with the modelling (NSRAC industry stakeholders-a, personal communication, January 2007).

In the light of this evidence, the pattern of interaction between the IMARES scientists and the NSRAC industry stakeholders at this stage in the process of advice production corresponded to a rather ‘linear’ division of labour; commissioned by the LNV, the IMARES modellers simply delivered an evaluation of the medium-term management advice produced by the NSRAC, without much involvement of the latter.

### **6.2.3 The request for an *ex-ante* integrated impact assessment**

The NSRAC had not changed the nature of their advice in the course of their interactions with IMARES modellers. The whole process of consultation with the NSRAC over the long-term management of the flatfish fishery thus came to a standstill and the conservation unit at DG-FISH decided not to postpone action any longer. In January 2006 they published a proposal for a harvest control rule to reduce fishing mortality year on year until a better regime of exploitation of the stocks could be achieved, presumably by 2015 (European Commission, 2005b). As I discussed in Chapter 5, the HCR constituted the core of the Commission’s official long-term MSY-based strategy for flatfish.



The fishing industry within the NSRAC felt alienated with the publication of the management proposal<sup>203</sup> – particularly after the way the Commission had neglected the consensual advice agreed formally by the NSRAC<sup>204</sup> (NSRAC, 2006c). Subsequently, they demanded that DG-FISH showed what biological and socio-economic evidence they held to support this precise proposal rather than any alternatives:

[T]he Commission has not been able to provide adequate advanced information on its proposals to the RACs, or discussed the key elements in such a way that the RACs could provide considered comment. If RACs are to deliver their full potential ways must be found for the Commission to explain the thinking behind its proposals in detail. This did not happen last year [2005] and we therefore hope that strenuous efforts will be made in this year to allow the RACs a genuine role in the pre decision-making stages of policy (NSRAC, 2006d:1-2).

Meanwhile, if any Member State was going to scrutinise the Commission's new proposal, that was meant to be the Netherlands. In the eyes of the LNV, DG-FISH had boldly overlooked their obligation to present sound evidence that the plan was

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<sup>203</sup> By contrast, the environmental NGOs within the NSRAC were informally satisfied with the DG-FISH conservation unit's attempt to move away from short-term risk-avoidance – i.e. doing precaution – and show a long-term ambition (NSRAC environmental NGO stakeholder, personal communication, February 2007).

<sup>204</sup> This also triggered criticism from the European Parliament in their review of the Commission's proposal:

The Commission rightly states in its explanatory memorandum on the proposal that the North Sea Regional Advisory Council is the principal forum for advice relating to the proposal. The NSRAC comprises representatives of the fishing industry and, *inter alia*, environmental organisations. It is striking that the Commission acknowledges that the NSRAC is the principal advisory forum but never subsequently refers to the NSRAC opinion. The only evidence on which the proposal is based is opinions delivered by two technical advisory bodies: the International Council for the Exploration of the Sea (ICES) and the Scientific, Technical and Economic Committee for Fisheries (STECF). In view of trends in plaice and sole stocks in the North Sea, it is right that the Commission should propose a management plan. But ignoring the opinion of the main forum where the Commission can consult the fishing industry and environmental organisations is surely a prime example of why the gulf between Brussels and residents of the European Union has grown so wide. Forums such as the NSRAC were established precisely in order to ensure that policies formulated in Brussels were not all too often produced 'about you, without you' (European Parliament, 2006:16).

reliable, making them wonder what all the fuss in Brussels about better regulation was really then about (LNV representative, personal communication, January 2007). In direct talks with DG-FISH, the Dutch government stressed the need for an *ex-ante* integrated impact assessment of whether the management proposal would achieve its targets for the fleets catching plaice and sole in the North Sea:

Our feeling was that we didn't have a clue of what this plan will bring us in the sense of length and depth. We saw that [fishing mortality] was going to go down very quickly; very rapidly within the first six, seven, eight years you would have a reduction of 50% of effort, which is rather a lot, that's rather a tough thing to discuss with the fishermen and [it was important] to find out whether you will have a sustainable fishery in the economic sense at the end (LNV representative, personal communication, January 2007).

DG-FISH responded positively to the demand for an *ex-ante* integrated impact assessment of the proposal (NSRAC, 2006b). They had not felt obliged to do it in the first place when they published the proposal in January 2006 but, nonetheless, with the arrival of the Better Regulation strategy<sup>205</sup> the Commission feared that they might not be able to get the flatfish proposal through the Council of Ministers in December 2006 without it (STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007). In addition, the long-term plan for flatfish fishery offered a good opportunity to get started with IIAs.

As a first step, DG-FISH decided to organise a meeting in May 2006 to prepare the ground for the IIA exercise, presenting the rationale of the proposal to the NSRAC and discussing with scientists, stakeholders and the Dutch government the scientific information available so far, what kind of questions needed to be explored further in the integrated impact assessment exercise, and how they could be addressed within a short time-frame. DG-FISH was ready to call for a special STECF working-group

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<sup>205</sup> Entering in force in January 2007.

after this meeting in order to run the IIA exercise. Indeed, the meeting in May 2006 served to establish the Terms of Reference for the STECF working group in charge of the IIA (DG-FISH conservation unit representative-b, personal communication, March 2007); and as a central aspect of the IIA exercise, DG-FISH announced that they would welcome for the very first time members of the NSRAC as observers to an STECF working meeting<sup>206</sup> (NSRAC industry representative-b, personal communication, April 2007).

All in all, the prehistory of the IIA discussed in this section recounts relevant milestones in the interaction between the different constituencies in the policy process before and after the publication of the long-term management plan proposal by the Commission. The request for the IIA proved the scientific appetite of the NSRAC industry stakeholders, a point I will develop below. Before that argument is fully unfolded though, I will need to turn to the IMARES model and how it was built.

### **6.3 A note on the design of the IMARES model**

In this section I interrupt the chronological flow of the chapter to address the approach used in the development of the IMARES model. The focal point of interest is that the model was far from an established tool at the time it was brought in for the IIA exercise. By and large, the IMARES model was developed in the course of exercises such as the evaluation of the NSRAC advice for the flatfish fishery that I discussed above. Around that time scientists at IMARES were planning to build a new modelling framework for management strategy evaluations, allowing for the exploration of ‘what-if’ scenarios in contrast to the hitherto dominant use of

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<sup>206</sup> Meanwhile, given the important role that science had played so far in the process of producing their advice to DG-FISH, the NSRAC insisted to the Commission on the need to encourage and facilitate scientific support to the RACs – for instance by providing the funding to commission their own research.

modelling for prognosis. In fact, IMARES was directly involved as a research institute in the development of the approach within the context of two projects funded by the EU 6<sup>th</sup> Framework Programme<sup>207</sup>. Therefore, the IMARES modellers made a non-trivial decision when they opted to use this framework still under construction in real policy problems to make more effective progress on its development (IMARES biologist-e, personal communication, March 2007).

Two immediate challenges stemmed from the process of development of the IMARES model. The first was related to the fact that the creation of the modelling framework followed an ‘open source’ approach. This implied that the modelling needed a critical mass of scientists to scrutinise it so that problems could be identified and debugged<sup>208</sup>. Those numbers were not yet in place and so the quality control of the IMARES modelling was in jeopardy, particularly in the view of those modellers within IMARES who were not fully supportive of the modelling philosophy:

[It is not] professional in the sense what I would expect [...] I’m a user but still those developers are basically, to my opinion only, they are amateurs, not professional developers. If a normal software firm hires developers what they produce is checked and double-checked and then they release it. And the next version, version 3.1, should be compatible with version 3.0. At least that’s what I, I was never a developer, but that’s what I expected. [Those developers] are very enthusiastic about writing all the software but the boring project of checking and double-checking, that’s something they say OK we don’t have time for that. [...] So they are [just] developing [...] To my opinion there is not much about checking, it’s what we users experience. And then we are going back to

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<sup>207</sup> EFIMAS (‘Operational Evaluation Tools for Fisheries Management Options’) and COMMIT (‘Committing to Tailor-made Long-term Fishery Management Strategies’), both projects funded by the European Commission’s 6<sup>th</sup> Framework Programme under ‘Scientific Support to Policies’.

<sup>208</sup> A modeller commented on the approach in the following terms: “As we get a larger community using it then also more and more people, bugs get reported. You have a wider range of settings and a wider range of people using it and kind of the things you never thought about come up. This is the open source theory which is called the ‘Cathedral and the Bazaar’” (IMARES biologist-f, personal communication, March 2007).

the developer and saying there is a problem here (IMARES biologist-b, personal communication, February 2007).

Yet, for the user of the model, playing this role was a real challenge. The IMARES model had a complicated internal structure due to its holistic ambitions and this created problems for its evaluation, as follows:

[In principle] I think [the model] is useful because it can show you the trade-off between different management strategies but the risk I see with these models is that they become more and more complex so that the transparency in terms of what are we actually calculating here becomes very intransparent [sic]. So, even to the scientist it is very difficult to understand what someone has been doing in one of these simulations because the amount of code that you need to go through and technical aspects of the way it's coded, you really have to know what you're talking about to understand what's happening. You have to go through it almost with a [code] interpreter and seeing what is the result of [each code] sentence. [In the end] you get a trade off from it but you don't know whether it's the right trade off [...] you sort of have to trust it's doing what it's supposed to be doing (IMARES biologist-b, personal communication, February 2007).

Trust was indeed the main resource that the scientists employed to validate the IMARES model. However, trust was sometimes lacking. Even within the IMARES institute's walls, some scientists were openly sceptical about what could be achieved with such a model, particularly in contexts where stakeholders were supposed to be involved:

As far as I understand, the idea behind is that we make and construct the 'animal'. The 'animal' is the fish stock and we tease that animal with fishing pressure, fishing selectivity, mesh sizes, regulation, all kind of regulations and then we look how the 'animal' behaves and what the output is [...] So what I do is I try to simulate reality to the utmost because I account for everything [...] So in the end, just like in the circus with the guy with a lion, I know how it behaves in any condition [...] I don't think this is very helpful because this is so much building a 'lion', a kind of an electronic lion, and then to learn from teasing the lion to understand its behaviour in all conditions [...] And then I come to a

mechanistic world, and electronic world like a computer game, if you press this then it does that, etc. (IMARES biologist-d, personal communication, January 2007).

All in all, the evidence shows that, even within the scientific community surrounding the IMARES institute, some actors were quite sceptical about the IMARES model and how useful it could be<sup>209</sup>. Moreover, how the IMARES model was somehow work-in-progress was openly disseminated in widely attended conferences<sup>210</sup> (ICES Symposium on Fisheries Management Strategies in Galway, observation, June 2006). This will offer a relevant backdrop for the following section.

#### **6.4 The NSRAC fishing industry's empirical ally: Engagement with the modelling in the biological subgroup of the IIA exercise**

Resuming the case study, I shall now discuss how the NSRAC flatfish industry stakeholders found an empirical ally in the IMARES model during the first meeting of the IIA exercise in Brussels.

To recall a few important milestones, the regulatory reforms in EU fisheries policy after 2002 placed the emphasis on enabling the participation of stakeholders in the advisory arena. The creation of the RACs that followed the reform was an attempt to remedy the alienation of the local stakeholders from the advisory process; scientific institutions such as ICES and STECF were definitively more open than they had been in previous years, when scientific advice was produced behind closed doors. Meanwhile, in 2006 the Better Regulation strategy was in the process of being

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<sup>209</sup> For a recent account of the biologists' uneasiness with Management Strategy Evaluations in general see Kraak et al. (2010).

<sup>210</sup> Which included, and this is important for the transparency argument, not only scientists but also policy-makers and stakeholders

established at DG-FISH. Subsequently, integrated impact assessments were introduced, which had to have at least some degree of stakeholder involvement.

The first IIA exercise to be conducted at DG-FISH was the one for the flatfish long-term management plan at STECF. Stakeholder participation was organised carefully for the first workshop of the integrated impact assessment in Brussels in September 2006. The NSRAC and the LNV had already been asked in May to contribute to the definition of the ‘Terms of Reference’ that should guide the IIA exercise. The opportunity to attend the entire first meeting that DG-FISH had offered to the NSRAC was mainly taken up by representatives of the Dutch and Danish fishing industries<sup>211</sup> (NSRAC industry representative-a, personal communication, January 2007). This presence of industry stakeholders produced *a priori* some mild disquiet among some of the scientists attending the meeting at STECF (DG-FISH conservation unit representative-b, personal communication, March 2007). Previous experience with the stock assessment process had shown that these stakeholders were not always helpful, according to views of some of those scientists<sup>212</sup> (STECF

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<sup>211</sup> The ‘green’ NSRAC member representatives attended only for a short time (NSRAC environmental NGO stakeholder, personal communication, February 2007). It is noteworthy that the environmental NGOs had considerably more financial struggles than their colleagues from the fishing industry at the time of attending NSRAC related events (see Degnbol and Wilson, 2008).

<sup>212</sup> Let us recall how issues with modelling were being approached at the time in the context of the stock assessments, triggering renewed concerns among scientists when it came to the IIA exercise:

It’s good to have the stakeholders around but it is also clear that there needs to be a certain regulation on their contributions; that they need to be constructive. I mean that they cannot destroy the work with their comments. So there needs to be some respect from each side, from the stakeholders and from the scientists, to listen to each other’s arguments and the arguments need to be sensible (STECF biologist, personal communication, April 2007).

Notably, the fishing industry had no problems to agree with that, at least in principle:

I think that most scientists are trying to do [modelling] as help and not as their own personal project. There is the odd mad scientist of course that just goes for the model without any connection to the world but I don’t really think that scientists are my major concern when we’re talking models. My major concern when we’re talking models is the users of the model, if you like, and that’s what I call the managers and the ‘greens’, because they don’t have the scientific knowledge or the fisheries knowledge. They have neither. And the fishermen can criticise a model based on his knowledge of the sea, not his mathematical knowledge. And a scientist can do it on a scientific background, even a mathematical

biologist, personal communication, April 2007). Meanwhile, for DG-FISH these issues warranted no more than some sort of warning at the beginning of the event:

Usually the first thing they do is make some sort of statement at the start of the meeting [...] It's usually attacking the Commission [laughter]. Then I, or whoever is representing the Commission, has to make an intervention and say look, well this is not the kind of meeting to have that sort of discussion. You're here to observe a scientific process. Please, contribute to the scientific process where you can, otherwise sit in the back and listen. And then usually they accept that (DG-FISH conservation unit representative-b, personal communication, March 2007).

Notably, not even a warning was needed this time in the end, "the stakeholders behaved quite professionally, without entering into the usual disputes with regard to

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background. A manager might, and I'm not saying all managers, but there's the danger that the manager takes the easy way out and just takes the model or the science 'hostage' if you like and I've seen that repeatedly. And managers of course do that for a reason [...] some managers are quite skilful in fisheries science, I think most managers are, but they work in a political system and they want to please their politicians [...] And if they can point to a model that says if we do that then this will be the outcome, they are happy. So that's the danger of models, it's not models *per se* (NSRAC industry representative-c, personal communication, April 2007).

Another NSRAC industry representative offered an example of what they considered was the environmental NGO's characteristic and enervating approach to modelling within the stock assessment process: "There is now a court case because the [outputs of the stock assessment] models are not being used" (NSRAC industry representative-b, meeting of the NSRAC Demersal Working Group in Copenhagen, observation, April 2007). He was referring to the World Wildlife Fund-UK challenge at the European Court of Justice to the legality of the Council of Ministers decision on 2007 cod quotas, which did not follow ICES scientific advice. The challenge was later refused by the European Court of First Instance (World Wildlife Fund, 2008). Meanwhile, the green organisations feared that it was too easy for the industry stakeholders to dismiss the outcome of a model or put pressure on politicians if they did not like the look of it. For this reason, they believed that it would be much better if only the scientists, as disinterested actors, discussed the outcomes of the models:

What comes out [of stock assessments] I don't think that should be negotiated. What you can negotiate is what kind of management you do want to have [...] if you're not happy with the rules or with the management, then you should change that and that is what has happened now [with the] negotiation on the long-term management plan [for the flatfish]. But once the plan is there you shouldn't negotiate on the outcome [of the scientific advice] (NSRAC environmental NGO stakeholder, personal communication, February 2007).



the scientists' work; they were keen to learn and contribute" (STECF biologist, personal communication, April 2007).

Following the Terms of Reference, at the outset of the meeting a representative of the DG-FISH conservation unit underlined that some modelling would be used, not to predict the future though, but to see whether the plan would make a difference to the *status quo* – also referred to as the 'baseline strategy' or as the 'no policy action' in the IIA guidelines (DG-FISH conservation unit representative-b, personal communication, March 2007).

As discussed in Chapter 5, during the course of the first meeting of the IIA exercise there were two different sets of actors, the biological and the economic subgroups, which worked in separate rooms, joining up only for plenary meetings. With only one exception, the NSRAC industry representatives decided to attend mostly the biological subgroup, where an IMARES modeller presented the eponymous model developed in the Netherlands. One of the main reasons for the NSRAC industry representatives' choice was precisely that this biological model was meant to be the cornerstone for the economic assessments, following the established linear division of labour in the advisory arena. Thus, attending the biological subgroup seemed to be a more effective use of their time (NSRAC industry representative-a, personal communication, January 2007). The biological subgroup was also where their influence and expertise might be more salient, as follows:

I could follow quite easily, although I am not a biologist, but the discussions, I mean. They basically said these scenarios are what we want to calculate and they put them in the computer and then it starts bubbling and something comes out of it, and then you can look at the interpretations of results. But the economic models I found them much more difficult because there were a number of models they used that I couldn't understand and there seemed to be a disagreement also between economists about what model was best and which data were best (NSRAC industry representative-a, personal communication, January 2007).

I was in the biological group for obvious reasons as I'm a biologist [by training] and that's usually where I have my influence, but I'm not a 'number cruncher', I'm there as a vulture just perching and waiting for other people sitting with their laptops and doing all the hard work, and then I have a statement on what they've done (NSRAC industry representative-c, personal communication, April 2007).

Hence, by prioritising the biological subgroup the NSRAC industry stakeholders were strategically choosing the side of the advisory arena where they had more expertise – notably, one fishing industry representative was a biologist by training<sup>213</sup> – and could have more impact on the outcome of the exercise; not least because of their much longer history of fighting the biologists than dealing with the economists<sup>214</sup> (NSRAC industry representative-c, personal communication, April 2007). The fact was that the biological subgroup offered opportunities for the NSRAC stakeholders and the IMARES scientists to learn and engage with each other, overcoming the 'you give me the questions, I then give you the answers' – that is, the 'linear' division of labour that had emerged in the past – as illustrated below:

To have [instead] a meeting with the [fishing] industry the day before and the day after the STECF session would have not been beneficial I think. Of course then it's only two days instead of five days perhaps, but, I mean [...] the good things [happen anytime]. So there's perhaps six hours of not really getting a sense of moving anywhere and there's half an hour or one hour of intense and very constructive discussion and then two hours of just hanging around again, and that's more or less so every day [...] I think there was a lot of good things going on. There were new people also, it's not just the same guys wearing a different cap; perhaps

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<sup>213</sup> Something not uncommon across stakeholder organisations heavily dependent on science (see Yearley, 1991, 1992) for the case of environmental NGOs).

<sup>214</sup> Yet, NSRAC industry stakeholders were not totally absent from the economic subgroup. In fact, it was the first time that they had a real serious discussion with economists (NSRAC industry representative-b, personal communication, January 2007). The economists welcomed the NSRAC, they had a deficit of data and stakeholder participation could help with elicitation of knowledge, as it happened indeed at some points: "For example, we didn't have very much data from the processing sector so we had one observer [NSRAC industry representative-b] who gave a 10-minute intervention on the processing sector [...] it still wasn't the hard data that we needed for our modelling but it certainly gave an insight" (DG-FISH economic unit representative, personal communication, February 2007).

representing the same institutions that go to ICES but other people. And every time there's a new person, there's a new angle, a new thought in it. Despite the fact that some people say that science is apolitical, that's crap, because when people are involved politics are involved as well. Opinions are involved. So it's not objective, science is never objective and therefore it's always interesting to have new people into a group because they see things perhaps in a slightly different way (NSRAC industry representative-c, personal communication, April 2007).

They have a scenario to compare with, the scenario of "doing nothing" but how do we define doing nothing? And [we] sat there and said well, it's not that complicated, is it? You just freeze the fishing mortality at the levels they are now and there was that huge discussion among the biologists about whether that would mean doing nothing and in the end we agreed that that would be a suitable scenario (NSRAC industry representative-a, personal communication, January 2007).

[Stakeholders] were aware of the facts that they could feed this model with, let's say, adaptations of the harvest control rule, because that is basically what I did during that week. I generated figures for this economic [subgroup] and on the other hand, those people from the industry, they came up with some alternative harvest control rules which I was able to simulate and see what kind of effects they had [...] I think the suggestions that they were making were quite realistic and sometimes it was good to follow their suggestions, to think about it and discuss it (IMARES biologist-b, personal communication, February 2007).

As it follows, even despite the particular idiosyncrasy of the modelling approach, there was no sign of critical inquiry into the IMARES model to uncover potential flawed assumptions about the fish stocks or the behaviour of the fishermen<sup>215</sup>. Instead, the industry stakeholders approached the model mainly as an instrument to

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<sup>215</sup> The following comment about IMARES model's design adds to the argument:

So we have presumed some extreme different scenarios for how our [Dutch] fishermen could behave, because how fishermen actually will behave is unpredictable [...] [We make] assumptions and if you go to the fishermen, they say no, and then they come with something much more complicated. Their behaviour is of course much more complicated than we assume but if we assume the extremes everything they do in between doesn't matter anymore if it doesn't affect the results too much (IMARES biologist-b, personal communication, February 2007).

structure discussion by comparing alternative scenarios and policy options in relative terms:

If you don't use a model you are talking on basic assumptions and you assume this and I assume that and we can talk a long way but the end of our discussions leads nowhere because it's like a general chat on the bar [...] So let's now [build a] model and use that as a basic reference for our assumptions [...] If it says that at the end I will have a [positive] cash flow of 3,221 euros, definitely that is not going to be the case, but for my policy I know it's definitely not going to be minus 50 [...] that is the direction (NSRAC industry representative-b, personal communication, January 2007).

In using the modelling this way the NSRAC industry stakeholders were able to cast doubt on whether the plan would make much of a difference compared to 'no-action' under the same initial conditions. The IMARES model suggested that the plan would work in biological terms<sup>216</sup>, but the comparison with the outcome of 'no action' was surprising:

The shocking thing was that the stocks would go in the same direction only a bit slower, you know, they would grow above the precautionary levels for the biomass and all the targets, and I found that really shocking and I thought, okay, so the Commission has this huge plan which has huge socio-economic repercussions for the fleet and basically a lot of economic pain but the outcome really is that it brings the stock to a certain level two years earlier, so why go through all the pain if nothing happens? (NSRAC industry representative-a, personal communication, January 2007).

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<sup>216</sup> Notably, this was briefly and partially validated by another biological model called ISH (after the German 'Institute for Sea Fisheries') that was mobilised for the IIA exercise and adapted for the flatfish fishery:

[IMARES] is more complex while my model is a simplistic, deterministic forecast model over 10 years, the IMARES model is a full feedback model so it takes also into account all the [biases in the stock] assessments [...] if you [eliminate the] bias then the model results of the IMARES and my model should be exactly the same. And I think that's what you see if you compare these two model outcomes (STECF biologist, personal communication, April 2007).

Thereafter, the NSRAC industry stakeholders believed that the Commission's management proposal would not add much value despite all the sacrifices that it would mean for the main flatfish fleet. DG-FISH reacted with surprise, according to the NSRAC fishing industry representatives:

The only difference that the Commission flatfish plan made was in timing, that by following the Commission's work-plan we might find, but not even with a high degree of certainty, we might achieve having the circumstances two years in advance of the timing that if we do nothing as just proceed as we do today [...] [So] the short outcome was that the Commission's proposal, from a biological point of view, will only make a change in timing, reaching the target two, three to five, maybe in exceptional circumstances ten, years sooner. [...] It sort of alienated the Commission immediately, and said, oh no, that can't be true. No? Why not? (NSRAC industry representative-c, meeting of the NSRAC Demersal Working Group in London, observation, October 2006).

This was quite difficult for the Commission to understand. I think it was quite embarrassing (NSRAC industry representative-a, personal communication, January 2007).

On this evidence, the NSRAC industry stakeholders succeeded in challenging the Commission's one policy option. This outcome was helped by the space offered by the biological subgroup and the IMARES model – one can say despite having been built using a framework that was still under development. The NSRAC industry stakeholders used the results of the IMARES model to legitimise other policy options that were reasonable in their view. In this sense, the IMARES model became the empirical ally that strengthened their authority in the policy process and also contributed to move beyond the previous 'linear' division of labour between the IMARES modellers and the NSRAC stakeholders reported at the beginning of the process.

### **6.5 The final decision on the long-term management plan for the mixed flatfish fishery in the North Sea**

The first meeting of the IIA opened up the number of policy alternatives when it should in fact have helped to arrive at a decision on one policy choice; the question that follows is how the decision-making process reached some kind of closure and whether the modelling had any role to play in this. As I shall discuss, closure came rather quickly after the first workshop of the IIA in Brussels in September 2006. Indeed, a decision was taken at the Council of Ministers in the end of 2006.

In hindsight, the DG-FISH conservation unit had not expected the IIAs to need so much development. The conservation unit referred to the scientific struggle experienced during the first workshop in the following terms:

We initiated [the IIA] but didn't finalise it [...] people really don't know very well what to do, they still have to develop the basic theoretical notions [...] it wasn't really possible to do an impact assessment in the short term that we believed [...] the work was not making much [of the] progress that we wanted (Meeting of the NSRAC Demersal Working Group in Copenhagen, observation, April 2007).

The LNV had similar feelings about the methodological aspects of the IIA. Yet they went further and expressed concerns about the very appropriateness of pursuing a long-term management plan under the current circumstances:

It's very difficult to get that impact assessment together because that also depends on a lot of modelling and very new modelling was done [...] [The exercise] gave us the idea that it would be very difficult to, in one step, determine the whole [management] process that will lead to MSY (LNV representative, personal communication, January 2007).

Meanwhile, for the NSRAC industry stakeholders, concerned as they were about whether DG-FISH's plan would make any difference, the IIA exercise did work. It

delivered evidence that other policy choices were possible, in line with a central feature of the Better Regulation strategy. As the convenors of IIA exercise acknowledged: “The conservation unit wanted the impact assessment to show that their plan was reliable so, on the first or second day [of the meeting], when we said ‘well, actually it is best to do nothing’, of course they weren’t very happy. But, at least from our point of view, we are trying to ensure this is something that the Commission uses to provide better regulation” (convenor-c, STECF SGECA-SGRST-07-01 meeting in Copenhagen, observation, March 2007).

The reassignment of authority towards the NSRAC industry stakeholders was clear. They had managed to establish themselves not simply as knowledgeable advisors but also as negotiating partners with the Commission as regards the shape of the management plan. Negotiations followed in the autumn of 2006 between the Commission, the Dutch government and the NSRAC – primarily the Dutch flatfish industry. DG-FISH’s focus on MSY returned as a major point of controversy in this new round of negotiations (NSRAC, 2006a). Industry representatives continued to claim that without formal discussions in the Council of Ministers on how to implement MSY it would be basically “putting the cart before the horse” (NSRAC industry representative-a, personal communication, January 2007). The LNV shared the same view. They were disappointed with the Commission’s ideas on how MSY could be operationalised, finding them too vague. In fact, they wanted to put MSY on hold: “We don’t know what MSY is, you can set an MSY reference level on a political basis but without any [clear] guidelines is a no-go in the political field so [...] the Dutch input was: no figures for MSY levels until we had a good discussion on that” (LNV representative, personal communication, January 2007). It is also important to note that the position of industry stakeholders in the NSRAC and the Dutch government was reinforced by the report from the European Parliament to the Commission, which stated that “it is premature to implement a method which has not yet been agreed within Parliament and the Council and to do so does not accord with the Community principle of ‘good governance’” (European Parliament, 2006:17, original emphasis).

Once again the policy process seemed at risk of stagnation in the autumn of 2006. Yet at this point two externalities played a strong influence on the course of the negotiations over the long-term management plan. Both the fish and the fishermen were bearing the costs of the long-lasting negotiations. Two years have gone passed and the conservation of the North Sea plaice stocks demanded even more urgent action according to the scientists and echoed by DG-FISH. Meanwhile, the fishermen – and particularly the Dutch long beam-trawling fleet – needed clarity over the regulatory framework to make decisions in a worsening context for their interests due to two major reasons. First, there were general elections in the Netherlands at the time and a serious chance of bringing in a ‘greener’ fisheries minister. In the view of the incumbent Dutch fisheries minister, an agreement had to be reached before a new cabinet could make things much more difficult:

There was a very strong feeling that the more environmental friendly parties or the ‘green’ parties in the Netherlands would win the elections and that we might not have, that we might find ourselves with a minister who’s actually going to agree with the Commission on their very kind of severe proposals for the Dutch industry. So it was a kind of strategic way, and also the Dutch industry said look we really want to have clarity now because it has been an issue which has been discussed for the past two years and we just want to know what to expect in the next year [...] And we were confident that this minister would stand up for us and reach an agreement that would at least mean, you know, although we always knew that the outcomes were going to be very harsh for us, it wouldn’t be that strict as the Commission eventually initially thought of [...] that only had a one-step approach. And we knew that our minister was going to stand by the RAC proposal, so we didn’t want to take the risk [of having another minister]. So we knew that if there was no decision in this December Council [2006], the next decision was going to be in, decision moment, was going to be in April [2007] and by then we could have a new minister, which could be a minister from the Labour party or from one of the green parties. As it turned out [after the elections] [...] the [new] minister is of the same party as the former minister so that’s okay, but we didn’t want take the risk (NSRAC industry representative-a, personal communication, April 2007).

Second, the economic conditions for the long beam-trawlers were deteriorating due to the sharp rise in fuel prices accompanied by a poor catch of the fish quotas due to



scarcity – i.e. higher costs and lower returns hitting at the same time. In view of these tough economic conditions the Dutch fleet urged that the new regulations for the flatfish fishery be settled as soon as possible so that they could begin planning ahead – they blamed the uncertainty over the regulation for the delay in their investment plans:

The situation, the fleet is going down, basically; our government [in the Netherlands] and we ourselves and also the [environmental] NGOs we want to modernise our fleet, make it more environmentally friendly and everything but we can't make any decisions now because we don't know what the catching opportunities are going to be for the future because of this plan and we don't know what's going to happen. So, basically, the industry said okay we can agree with a decision now so that there will be a management plan as for the 1st of January 2008 because, for us at least, it brings some certainty of what's going to happen the next couple of years. And of course the plan will have huge implications and we all know that, but at least there is some kind of certainty about what's going to happen in the next coming years (NSRAC industry representative-a, personal communication, January 2007).

By and large, acting against these two externalities marked a tipping point in the negotiations between the Dutch government and fishing industry with DG-FISH. An agreement could be reached without waiting for a new IIA exercise so that a clear regulatory framework could be established as soon as possible, as expressed by NSRAC industry representative-a: “We said, well, the problem we have at this moment is that there is so much uncertainty because these talks [over the management plan] have been on for months and months and months” (personal communication, January 2007). The NSRAC fishing industry representatives demanded medium-term stability and flexibility with long-term targets in the management approach so that they could be fixed at a later stage. At the LNV they

aligned themselves with the (Dutch) fishing industry's vision of two distinct steps<sup>217</sup>, as follows:

[With] the first stage [...] you give the fishermen a very clear signal: okay in the first step you really going to have to suffer, the first step is going to bring us to [precautionary levels in the spawning stock biomass] and then we are going to think about how are we going to reach an MSY level which we are going to revise as well by that time. So you really have quite a well-defined first step without defining the whole way of how you are going to reach MSY. And that creates the first vision for the fishermen of what is going to be like the first five, six or seven years [if recruitment does not fail dramatically in the meantime] [...] and they can base their economic choices on that vision without the *ad hoc* every year 'maybe it will get better next year' (LNV representative, personal communication, January 2007).

Meanwhile, the DG-FISH conservation unit was in agreement in not wanting to wait for a new IIA exercise. After two years of talks, and with the flatfish stocks in need of urgent action, they were also willing to reach closure with the plan:

The Dutch industry has come to realise that, well, what they want to have is a clear picture of where things will come, so the sooner we have the picture the better. So, in the end, they changed their position and said, well, even if the impact assessment is not finalised, let's try to agree on it as soon as possible and that is what, what kind of unlocked the whole process [...] [They] said well perhaps if we divide the plan into two phases... Let's do the following, let's just establish the first phase, the mid-term objective of bringing the stocks within 'safe biological limits' and for that we don't need socio-economic evaluation. In the meantime, let's try to make progress slowly on this kind of [IIA] methodology so, by the time we have reached the mid-term objective [and then be ready to start the second phase], we will have been able to have a much better socio-economic evaluation of the long-term part [...] [since] we will do so on the basis of a much more sophisticated and complete socio-

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<sup>217</sup> It is worth noting that, at the time of research, the then recently approved management plan for the Bay of Biscay sole also had the same two-step approach and both the Dutch government and the European Parliament had previously waived the need for policy consistency across the EU as a necessary guiding principle (LNV representative, personal communication, January 2007; European Parliament, 2006).

economic analysis than the one we [have initiated]. So, at the end, the compromise we have to arrive to is a mid-term recovery objective in terms of the ['safe' stock] levels, and then we basically try to find a combination of what is sticking to our principle that the overall management of fishing resources should be around MSY and, at the same time, provide sufficient time and elements for a possible revision of those objectives. In other words we safeguard the principle that we should go to MSY but we provide mechanisms in the regulation that provide for the possibility of reviewing certain aspects of the long-term plan (DG-FISH conservation unit representative-a, personal communication, March 2007).

The negotiations were finalised at the December Council of Ministers in 2006 and led to a final version of the plan that stipulated this two-step approach. This first phase could be formally considered an emergency measure for the flatfish stocks, which were in urgent need of action in the view of the Commission (NSRAC, 2006a), and therefore the IIA could be (legally) skipped. At the same time, this first stage should allow more time to establish the scientific methodology for IIAs (DG-FISH conservation unit representative-b, personal communication, March 2007), and thus the second phase could benefit in due course from “the basis of a much more sophisticated and complete socio-economic analysis than the one we could have done in the [early] autumn [during the first STECF meeting]” (DG-FISH conservation unit representative-b, personal communication, March 2007).

The administrative and legal work pushed the official publication of the long-term management plan into summer 2007 (see Council of the European Union, 2007). DG-FISH acknowledged the central input of the NSRAC in the final outcome (NSRAC, 2007c). And the fishing industry in the NSRAC certainly celebrated that their two-step approach had finally made it into regulation<sup>218</sup> (see NSRAC, 2007d).

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<sup>218</sup> It is noteworthy that the environmental NGOs in the NSRAC did in principle support the introduction of a long-term management plan but expressed their fear that it did not sufficiently address the problem of high discards in the 80mm flatfish fishery (World Wildlife Fund, 2006b). In this respect, they had been demanding an increase in the ‘minimum landing sizes’ with no success so far (NSRAC environmental NGO stakeholder, personal communication, February 2007). It is also

Moreover, although they did not like the MSY targets included in the plan without a full socio-economic assessment, they welcomed the Commission's acknowledgement in the regulation that the MSY targets were flexible and subject to mid-term revision (Meeting of the NSRAC Demersal Working Group in Copenhagen, observation, April 2007).

The evidence presented shows that the IIA did not bring the policy process to an end in rationalistic fashion. As a new space domesticated by the NSRAC industry stakeholders, the IIA exercise enlarged the political arena. It triggered social learning and redistribution of authority and, in so doing, opened up the policy process. The basis for the closure came from addressing the two negative externalities created by the long-lasting negotiations between DG-FISH, the NSRAC flatfish fishing industry and the Dutch government. Both the fish – especially the plaice stocks – and the actual fishermen – the Dutch long beam-trawling fleet – had been bearing the costs of the on-going policy process.

## **6.6 Conclusions**

This chapter has offered a step forward in the analysis of the establishment of the new sociotechnical framing following from the reform of the CFP in 2002. In this respect, I have approached the integrated impact assessment exercise – the second element of the troika of instruments – as a distinctive policy instrument. In so doing, I have focused on the biological subgroup at the first workshop of the IIA exercise, where fisheries management options rather than simply biology was discussed. I will outline two major points.

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worth noticing that, concurrently with the negotiations for the long-term management plan, the Commission was starting to consider banning discards (The Fisheries Secretariat, 2007).

First, while the biological modelling became the object of inquiry for the economists in their sub-group, the evidence in this biological sub-group points to a different direction. The IMARES model assisted the actors as a heuristic to compare the biological effects of the implementation of two alternative policy strategies – the long-term management plan and the ‘status quo’ scenario. Notably, the NSRAC industry stakeholders engaged with the IMARES model as an ‘empirical friend’ (after Yearley, 1991, 1992) that enabled them to legitimise the alternative policy option – that of no-plan. By and large, they did not engage in any scrutiny of this model despite its shaky foundations and their high stakes in the IIA exercise – in contrast to what it could have been expected from the experience of other contexts (see Yearley, 2006). And this was not only the case for the NSRAC industry stakeholders but also for the NSRAC environmental NGO representatives, who had expressed some of them an opinion about the long-term management plan proposal for plaice and sole published by DG-FISH (World Wildlife Fund, 2006b), as well as about the stock assessments on that year (World Wildlife Fund, 2006a), but did not engage much with the IIA exercise – let alone the IMARES model<sup>219</sup>.

Overall, the IMARES model played a different role across the two different subgroups – economic and biological – established in the first meeting of the IIA exercise. Furthermore, the role of modelling at large was framed differently depending on whether it was a stock assessment exercise or an IIA exercise. In other words, the degree of scrutiny and the usefulness of the modelling were context dependent<sup>220</sup>.

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<sup>219</sup> At the same time, it is important to stress that the environmental NGOs generally struggled to keep the pace with the different issues that demanded their attention (see Degnbol and Wilson, 2008).

<sup>220</sup> In this sense, it is interesting to reflect on participatory modelling exercises based on focus groups, which can often create a context of their own (see Yearley, 2006). This was also my experience of one of those exercises, which I observed during my fieldwork (EFIMAS Project – Northeast English Focus Groups in York, observation, August 2006).

Second, this chapter has accounted for the introduction of the Better Regulation strategy and the IIA instrument, which offered a chance for the NSRAC industry stakeholders and the fisheries biologists to explore a division of labour away from the hostility of the stock assessments in the first place, but also away from a ‘linear-sequential’ pattern of interaction. The IIA exercise became constitutive of the dynamics of establishing a division of labour between the NSRAC and the fisheries scientists, that is, of the efforts to put up a new sociotechnical framing for decision-making in EU fisheries management. At a point in time when the scope and resources of the NSRAC as a provider of sound advice had yet to be established, both constituencies shifted from a ‘mechanistic’ relationship to a more ‘organic’ one (Burns and Stalker, 1994[1961]). As the co-production framework illuminates, the IIA exercise did not only provide an alternative fisheries management option but also emerged as a space to redefine who-had-authority-when in the policy process and, in this respect, one for social learning and the reassignment of authority.



## **7 Building a new framing for the North Sea flatfish fishery: The introduction of harvest control rules**

### ***7.1 Introduction***

In Chapters 5 and 6 I looked at the introduction of the integrated assessment modelling and the integrated impact assessment tools in the management of EU fisheries by means of a case study of the North Sea flatfish fishery. In order to complete the empirical analysis of what I have been referring to as the troika of instruments to depoliticise EU fisheries management decisions, in this chapter I will throw the spotlight on the implementation of the harvest control rules – the pinnacle of the reframing efforts.

In the marine domain of the EU, but also neighbouring fishing waters such as the Norwegian Sea and Barents Sea, the shift to medium-term and long-term management strategies such as the one introduced by the European Commission in the 2002 reform of the CFP was operationalised by the introduction of harvest control rules. Yet this was easier said than done. I shall show that the first time the HCR for the North Sea flatfish fishery was applied it did not deliver the depoliticisation that it was meant to bring. Yet, evidence from the North Sea flatfish case was necessarily scarce to say anything of some general significance about how HCRs worked given the fact that the HCR had just been negotiated and approved at the time of research. The observed outcome could have simply responded to reasons of contingency. This is why I will ‘turn to Norway’ in this chapter.

Looking for contrasting evidence to that of the North Sea, between April and June 2007 I paid a field visit to Norway, considered a benchmark in fisheries management. From the outset, I embarked myself on a case study of the North East Arctic (NEA) cod fishery in the Barents Sea, managed jointly by Norway and Russia. As it happened, an HCR for the NEA cod fishery had been approved in 2004 to automatise the process of reaching decisions about quotas. It was the first HCR



introduced by the ‘Joint Norwegian-Russian Fisheries Commission’ (JNRFC) and looking into how it was working three years later turned out to be timely, as I shall describe.

As follows, in this chapter I will introduce the case study of the implementation of the HCR for the NEA cod fishery in the Barents Sea and reflect on what kind of insights can be gained for the main case study from this different, but not too dissimilar, policy arena for fisheries management. Indeed, the HCR for the NEA cod fishery presented a number of commonalities with that of the North Sea flatfish fishery. First, in both cases the policy decisions involved more than one country. Second, in the EU, as well as in Norway and the Russia Federation through the Joint Norwegian-Russian Fisheries Commission<sup>221</sup>, policy-makers were looking for mechanisms to put to rest the political trading or bargaining taking place every year over the establishment of the catching quotas for shared stocks. HCRs became the preferred solution in both settings. Third, in both cases the HCRs were conceived to provide not only biological but also economic sustainability, the latter being oriented to reduce the fluctuation between the quotas across several years and, thus, to benefit the fishermen by offering them stability to run their own businesses.

This chapter will put forward two related arguments. First, I will show that, while the HCRs were meant to depoliticise the setting of the quotas, neither HCR delivered at the time of cutting down political trading. In the case of the HCR for the NEA cod, I shall present how it was not prepared to withstand the growing numbers of illegal and unreported catches in the fishery later on estimated. Meanwhile, in the case of the HCR for the North Sea flatfish I will describe how Norway, which had a modest slice of the plaice annual quota but still a decisive say over the management of the stock, raised concerns over the design and implementation of the HCR. Second, I shall argue in this light that the implementation of the HCRs provided in both cases a

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<sup>221</sup> The organisation where decisions about the NEA cod stock were made.

political space for social learning and reassignment of authority – that is, for political work.

The structure of the chapter is as follows. In §7.2 I will present the case study of the operation of the NEA cod fishery. I will do so in four steps. First, I will introduce the general context of the NEA cod fishery. Second, I will describe how an HCR was established for the NEA cod fishery to depoliticise management decisions in the JNRFC. Third, I will address how the HCR for the NEA cod fishery worked in practice in the following few years after its approval in 2004. In particular, I will focus on the disruption to the operation of the HCR introduced by the scale of illegal and unreported catches carried out by the Russian fleet. Fourth, I will point out that, contrary to how it was originally envisaged, when to apply and how strongly to enforce the HCR turned out to be political decisions in the light of the illegal and unreported fishing. In §7.3, I will go back to the case of the North Sea flatfish fishery and account for how the HCR for the flatfish fishery was implemented immediately after it was officially approved. As with the case of the HCR for the NEA cod fishery, I will show that the application of the HCR did not cut down political work but offered instead a space for redistribution of authority. Finally, in §7.4 I will offer some conclusions.

## **7.2 *The introduction of the harvest control rule for the North East Arctic cod fishery: A supplementary case study***

### **7.2.1 *Setting the stage for the case study***

The North East Arctic cod<sup>222</sup> species certainly stands out in the Barents Sea. First, unlike the cod fishery in the North Sea – often involving discards of other species

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<sup>222</sup> *Gadus morhua* in its scientific term, it is also commonly referred to as ‘skrei’ in Norway. The NEA cod stock spawns in the spring along the Norwegian coast, especially around the Lofoten Islands and then the offspring cod swims to the Barents Sea, where they live until they come back with their spawning migration.

(Symposium on Cod Recovery in Edinburgh, observation, March 2007) – the NEA cod stocks are less mixed with other stocks and, therefore, the fishery is relatively free from the problem of by-catch (NFA<sup>223</sup> representative, personal communication, May 2007). Second, in an economic sense, the NEA cod stocks are the most important of the Barents Sea, providing the largest share of profits for the fishing activity<sup>224</sup> (Norwegian College of Fishery Science economists-a-b-c, personal communications, June 2007; Norwegian Fishing Vessel Owners Association, personal communication, May 2007). In close relation with the economic importance of the NEA cod, this species has been extensively studied by fisheries scientists, particularly over the second half of the 20<sup>th</sup> century and beginning of the 21<sup>st</sup>. Research has been taking place both in Russia and Norway by virtue of the long-standing cooperation between the ‘Institute of Marine Research’ (IMR) in Bergen, Southern Norway, and the ‘Polar Research Institute of Marine Fisheries and Oceanography’ (PINRO, for its Russian acronym) located in Murmansk<sup>225</sup>, Northern Russia.

Let us briefly touch upon this collaboration, significantly relevant for the context of this case study<sup>226</sup>. At the time of research in 2007, IMR and PINRO were celebrating their 50<sup>th</sup> anniversary. It all began back in 1957 when a scientific vessel from PINRO visited Bergen<sup>227</sup>. A scientific vessel from IMR returned subsequently the visit to Murmansk in 1958. From then onwards, a series of regular meetings were held between scientists from the two organisations (IMR biologist-a, personal communication, May 2007). In 1965 a joint research programme for demersal

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<sup>223</sup> Standing for Norwegian Fishermen’s Association.

<sup>224</sup> In this respect, fisheries advice in Norway takes seriously into consideration the availability of food for the cod species, a predator of the demersal waters: “One should be careful not to harvest too much of the pelagic species, that is, capelin, herring, that are the basic important food for cod” (Norwegian College of Fishery Science economist-a, personal communication, June 2007).

<sup>225</sup> Ever since the days of the Russian Revolution in 1917, the city of Murmansk on the Kola Peninsula has functioned as the hub of the Russian northern fishery basin, the second in importance in the country behind its far eastern fishery basin (Hønneland, 2007).

<sup>226</sup> For a thorough analysis, see Hammer and Hoel (2012).

<sup>227</sup> In other words, most of their cooperation had happened across the Iron Curtain.

species such as cod and haddock started, later on extended to the capelin stock. Some years later the Norwegian and Russian<sup>228</sup> scientists also began doing joint surveys, exchanges of data – for instance, a joint database on fish stomach content – and to hold a formal meeting between scientists from both institutions every year around spring. In addition, in the 1980s the scientists from both countries started a series of joint symposiums every two years. Apart from these scheduled meetings there were a lot of less formal exchanges throughout the year: “We travel to Murmansk or scientists from there travel here to keep the cooperation going and check that we are treating the data in the same way. [We are] trying to improve quality and do science together [...] that also involves exchanging technicians of course” (IMR biologist-a, personal communication, May 2007). Although, with time the cooperation broadened to other issues such as shrimps, seals, plankton or oceanography, the collaboration was stronger on the fisheries side because their work was directly feeding the ‘Arctic Fisheries Working Group’ (AFWG) in ICES.

All these scientific exchanges had an echo in the political arena with official contacts between the governments of the two countries, which ended up in an Agreement for Cooperation on the 11<sup>th</sup> of April 1975 that established the Joint Norwegian-Russian Fisheries Commission to promote fisheries cooperation on the rational utilisation of the shared marine resources and further coordination of the scientific investigations<sup>229</sup>. Meeting annually since 1976, these formal JNRFC sessions have involved members of the two countries’ fishery authorities, ministries of foreign affairs, marine scientists and representatives of the fishing industry (IMR biologist-c, personal communication, May 2007).

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<sup>228</sup> Part of the Soviet Union at that time.

<sup>229</sup> The JNRFC defines objectives and practices for cooperative management between the two states within the fields of research, regulation – TACs and technical measures – and compliance control (The Norwegian Ministry of Fisheries and Coastal Affairs, 2011).

Soon after its creation the Joint Norwegian-Russian Fisheries Commission became the management body of the NEA cod, haddock and capelin stocks in the Barents Sea (Hammer and Hoel, 2012). Following the rise of the new oceans regime at that time, multilateral negotiations for the Barents Sea fisheries under the governing body of the ‘Northeast Atlantic Fisheries Commission’ (NEAFC) changed for face-to-face negotiations between coastal states with fishing rights over the fish stocks under the Exclusive Economic Zones of 200 nautical miles<sup>230</sup>. As follows, the fish stocks could only be properly managed through agreements between the parties directly concerned.

By the early 1980s the cooperation had taken shape and the two parties were able to agree on the quota sharing of the joint stocks in the Barents Sea<sup>231</sup>. Meanwhile, cooperation in compliance control arrived later in 1993 (Hammer and Hoel, 2012), and consisted of the exchange of catch data and inspectors as well as the harmonisation of national enforcement routines – since implementation of the TAC depended on each country (see also Hønneland, 2007, 2012).

Yet, within the JNRFC, Norway and Russia represent two very different approaches to fisheries management. In Norway the management authorities have a long history. Norway established one of the first fisheries administrations in the world, the Norwegian Directorate of Fisheries, in Bergen in 1900, and the first Ministry of Fisheries and Coastal Management in 1946 (Hersoug, 2005). The Ministry is based in Oslo and is responsible for the administration of the whole sector in Norway but has delegated most of the actual implementation to the Norwegian Directorate of

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<sup>230</sup> Two hundred mile EEZs were established in 1977 in Norway and in 1984 in the Soviet Union – although the latter established a 200-mile fishing zone in 1976 (Hammer and Hoel, 2012). Notably, the two countries did not manage to agree on a boundary line separating the two EEZs in the Barents Sea until 2010. An interim arrangement known as the Grey Zone was in operation between 1978 and 2010 “to provide for enforcement of regulations against third countries in the disputed area of the Barents Sea” (Hammer and Hoel, 2012:251).

<sup>231</sup> Cod and haddock stocks are shared on a 50–50 basis, while the capelin stock is shared 60–40 in Norway’s favour (Hønneland, 2007).

Fisheries, located in Bergen. While the Ministry is responsible for policy-making and planning, the Directorate of Fisheries takes care of the day-to-day measures, producing a vast number of specific regulations per year<sup>232</sup>. The Directorate is also responsible for licensing of the fleet, authorisation of annual catching rights and quota control. Norway was also one of the frontrunners in establishing modern scientific research in fisheries, first through the Norwegian Directorate of Fisheries and later through the Institute of Marine Research<sup>233</sup>, created in Bergen in 1900 (Hammer and Hoel, 2012). Meanwhile, the Norwegian Fishermen's Association, an umbrella organisation that dates back to 1926, has also played a strong role in the management of the fish resources<sup>234</sup>. The NFA collaborates in full partnership with the government authorities<sup>235</sup>, to the extent that it has an active role in by sponsoring

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<sup>232</sup> Fisheries management in Norway is extremely rich in rules and regulations, as pointed out by Jentoft and Mikalsen (2004).

<sup>233</sup> In fact, this was a subdivision of the Directorate until 1989 (Hammer and Hoel, 2012).

<sup>234</sup> Fishermen are represented in the association by their county branch of the association or group organisation with the exception of the coastal fishermen, who parted away from the mother organisation in the early 1990s. It is also relevant to mention that:

Despite strong involvement from the Fishermen's Association in rulemaking, there is still quite a distance—geographically and psychologically—from the Association delegates to the 'man in the boat'. What finally becomes the Association's position on an issue is often a fragile compromise that is bound to frustrate many of its members. Therefore, when fishermen at the grass roots level criticise the management system, their main target is often the Association [...] By co-opting the Association in the management process, the government avoids some of the pressures and criticisms it would otherwise have had to face (Jentoft and Mikalsen, 2004:130, emphasis in the original).

<sup>235</sup> The fishing industry throughout history has been a strong political force in Norway. For instance, their opposition was believed to be a decisive factor behind Norway's rejection in referendum to join the European Community in 1972 as well as in 1994 (Lequesne, 2004). Notably, some scholars highlight a steady process towards more delegation of management to the fishing industry as well as towards fishermen self-management:

The NFA was established in 1926 and organizes both boat owners and crew and is the biggest stakeholder organization in the fishery industry. The NFA can today be considered an integral part of fishery management as opposed to just an outside interest group. This is reflected in the way that they have collaborated with the Ministry of Fisheries and Coastal Affairs about compromises on quota policy, fleet structure and management issues. This is worth noting because quotas are not normally allocated on the basis of annual political decisions in Norway. Instead, shares of the Norwegian TAC in both the pelagic and demersal fisheries are allocated on more of a long-term basis to different groups of vessels that stem from politically approved allocation principles. The allocation principles are often a result of negotiations within the NFA. The organization has significant influence in political matters and has contributed to form the system, but has also accepted that the individual fishers have

and designing new rules and regulations<sup>236</sup> (Gullestad et al., 2013; Jentoft and Mikalsen, 2004).

Fisheries management in Russia has a very different history by contrast. In line with other areas of public policy, fisheries management in Russia underwent significant changes after the dissolution of the Soviet Union (Hønneland, 2005). The new institutional framework that resulted from the establishment of the Russian Federation in December 1991 reduced the status of the policy agency responsible for

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got more responsibility for their own conduct (Johnsen, 2013:11; see also Johnsen et al., 2009).

<sup>236</sup> During my fieldwork the fishing industry stakeholders in Norway often expressed their pride over their management system and how it worked. Indeed, “most Norwegians in the industry have the impression that Norway is among the frontrunners in fisheries and aquaculture. This does not apply only to boats and gear [...] but includes fisheries management as well” (Norwegian Fishing Vessel Owners Association representative, personal communication, May 2007). At least at the time of research, even some of the environmental NGOs shared this national pride in Norway to a certain degree (environmental NGO representative, personal communication, May 2007) and Norway indeed came out at the very top in the international rankings (Pitcher et al., 2006). There was also fluent dialogue between the fishing industry and the scientists (NFA representative, personal communication, May 2007; Directorate of Fisheries representative-a, personal communication, May 2007; IMR biologist-b, personal communication, May 2007) as well as between the fishing industry and the policy-makers (Directorate of Fisheries representative-a, personal communication, May 2007; Norwegian Fishermen’s Association representative, personal communication, May 2007; see also Hersoug, 2005; Gullestad et al., 2013; Jentoft and Mikalsen, 2004). Even between the fishing industry and the environmental NGOs dialogue had made its way:

We have, I would say, a very good relationship with the industry now compared to what we had. Six years ago the Norwegian industry in particular they hated us because of the earlier days with the whaling campaign. There was absolutely no dialogue with the fishing industry and that goes with the government as well but that was after many years of anti-whaling campaigning we were not very popular in Norway. But then we’ve been working on this. Now we have a good cooperation with the Norwegian Fishermen’s Association, for example on things like pollution, and oil and gas. When it comes to particular fishing management issues there are of course, some disagreements but I would still say that we have a good dialogue (environmental NGO representative, personal communication, May 2007).

All in all, these actors committed under all circumstances to the goal of making Norway the ‘gold medallists’ in responsible fisheries management. However, it was not that easy to be champion of sustainability in a context where several fish stocks were shared with other countries:

The most important aspect of the Norwegian resource situation is that more than 90% of the total catches originates from shared resources [...] In practical terms this implies that any ‘world champions’ in resource management would have little impact if other partners do not follow suit (Hersoug, 2005:18, emphasis in the original).

fisheries from that of a ministry to a state committee<sup>237</sup>. This marked the beginning of a rather shaky period, as follows:

The process of reformation in the fisheries management system has lasted for at least 15 years already. When the old Soviet structure was eliminated the fisheries were performing chaotically. The rules of the game were extremely unstable and were changing constantly. Almost every year new documents were experimented by the government, some of them contradicting to the previous ones. This situation continuously forced fishers to invent new schemes to adapt to the situation and find new ways to survive. The main fisheries management authority at the federal level changed its institutional identity 6 times during 1990s and was transformed from an all-embracing Ministry of Fisheries into the State Fisheries Committee [...], which was substituted by the new institutional establishments in 2003. There have been 10 different leaders of the federal body during the last ten years, only two of them were professionals having appropriate (fisheries) background (Ivanova, 2005:67).

Therefore, Norway and Russia have been arriving at the negotiating table of the JNRFC in significantly different contexts – one very stable, the other in constant shift<sup>238</sup>. This inevitably transpired over issues at the annual JNRFC meetings:

The tradition is so that this Joint Norwegian-Russian Fisheries Commission, they have always used the ICES advice as basis. Up to now at least. And at least here in Norway there's no discussion about that [...] I think it goes without saying that Norway will base all their work in negotiations like that on the ICES advice. It would be quite impossible to do anything else. In Russia it may be a bit more complicated because

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<sup>237</sup> Hønneland and Jørgensen (2002) add light to this point, as follows:

Ministries and state committees are various types of administrative bodies at the federal levels. The ministries are placed higher in the political hierarchy since their leaders are members of the Government, but state committees are not subordinate to any ministry. Therefore, the federal body for fisheries management became a lower level authority after the dissolution of the Soviet Union, but remained an independent administrative body (2002:361).

<sup>238</sup> And perhaps, not surprisingly, a low score in the international rankings for responsible fisheries management (Pitcher et al., 2006).



there are many, there are big changes taking place all the time in the structure of fisheries society and quite often they have changed their delegation leaders for instance and they come from various parts of the political structure there, and such changes might also influence how strongly they will depend on the ICES community (IMR biologist-c, personal communication, May 2007).

Still for years the JNRFC has presented itself as an exemplary way to manage the fishery, with low levels of conflict between the two countries (Halffman, 2008). This was indeed the case during the Soviet period, although for the very particular reasons, as follows:

If you go back to the Soviet period where the costs and an income structure was completely different because it was ruled not by Western market economy [...] For example, the Russians had their prices to the fishermen per kilo of fish set in the early 50s and when they met a new species then they would tend to have a – like blue whiting for example, which was not fished in the 50s – had a much higher price to fishermen than cod because that price which was established in the 60s and 70s catered also for higher catching costs. So there was a lot of peculiarities that made quota trade possible and both parties within their systems won. We could give away a lot of blue whiting to them and we could get some cod back. This doesn't happen anymore at all because now we are in the same world economy (Directorate of Fisheries representative-a, personal communication, May 2007).

This situation did not last through the 1990s though. After the collapse of the Soviet Union the Russian fishermen from Murmansk became more interested in the very profitable cod stock and, overall, the region became more economically dependent on the Barents Sea fisheries. Things became particularly less straightforward when the NEA cod stock levels appeared to be on a downward trend at the end of the decade and both ICES and the JNRFC had adopted the precautionary principle<sup>239</sup>, which in practice implied larger target levels of spawning stock biomass. All this

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<sup>239</sup> The JNRFC adopted the precautionary principle in 2000 in fact, two years after ICES (Hammer and Hoel, 2012).

amounted for significant decreases in the TACs advised by ICES (Halffman, 2008), and several times the JNRFC established quotas well above the recommended TACs<sup>240</sup> because of the opposition from the Russian authorities to reductions in the catching opportunities for the Russian fishing industry (Hønneland, 2004). Notably, Russia accused Norway of having hidden economic reasons for supporting lower TACs; Norway, being a wealthy country, could afford to maintain low quotas and encourage high world-market prices for cod so as to force its Russian competitors out of the market (Stokke, 2005). Often the Norwegian delegation conceded higher quotas to Russian demands<sup>241</sup> since the cost of no-agreement on the quotas – with the potential ending of the bilateral management regime between the two countries – was too high (Directorate of Fisheries representative-a, personal communication, May 2007). In order to justify the overseeing of the scientific recommendations, the Norwegian authorities came up with the ‘pity-the-Russians discourse’, feeding the Western perception of Russians as ‘poor’ and that Norway was acting on humanitarian grounds (Hønneland, 2004).

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<sup>240</sup> It is important to grasp a glimpse of how the JNRFC meetings to set up the TACs were run:

During these Commission meetings, the first day is always set aside for the scientists to present and explain the situation for the various stocks that they deal with. And what we do then is present the advice from ICES. So, in a way, I have the ICES hat on my head. We present what is the advice on the stock and then we try to go a bit further, we explain why it's the advice just like this [...] And then we are present there for them to ask questions [...] you know this Commission meeting is every second year in Russia and every second year in Norway. And there are scientists from both parties. And we, we have a tradition that when we are in Norway the Norwegian scientists have the main role. They do the presentation and explanation and then the scientists from Russia just add some comments to this and when we are in Russia, it's the other way around [...] But it's always the ICES advice that's explained. It's not the Norwegian view, or the Russian view but the ICES view. And the rest of the meeting we do almost nothing. I mean, when the negotiations are going on, that's mainly between the [political] leaders (IMR, biologist-b, personal communication, May 2007).

<sup>241</sup> As evidenced by the fact that, throughout the 1990s, seven out of ten quotas exceed ICES recommendations (Rønning, 2002). In justifying back home the establishment of quotas above the TACs, the Norwegian authorities often raised socio-economic concerns, such as “the importance of these fisheries for the economically disadvantaged population of North-western Russia” (Stokke, 2005:1; see also Hønneland, 2004).

By and large, with the turn of the 21<sup>st</sup> century the balance of socio-economic and biological interests at the time of setting the quotas for the NEA cod had become a matter of political trading. Similarly to the case of North Sea flatfish fishery, this led to the establishment of a harvest control rule for the NEA cod. I will address this process in the next subsection.

### **7.2.2 Establishing the harvest control rule for the North East Arctic cod fishery**

Following the adoption of the precautionary principle in 2000, the Joint Norwegian-Russian Fisheries Commission agreed to provide in 2002 some degree of inter-annual stability in the TAC for the NEA cod by means of a harvest control rule. The HCR was the way to grant that the TACs would not fluctuate dramatically providing that the estimated fishing mortality remained within precautionary levels. Therefore, the JNRFC pursued both to preserve the biology and to allow the industry for better predictability of the catching opportunities for the following year. In addition, this should ease decisions about the quotas at the JNRFC.

As in the case of the North Sea flatfish fishery, the process of designing a harvest control rule and establishing a management target to aim at took around a couple of years:

We try to find out what do the fishermen and what do the politicians and what do the society want from this cod stock or the management of this cod stock [...] They don't want it to be depleted. This is taken care of by the [precautionary approach]. They want to harvest as much as they can. And then you also want stability. The fishermen and the industry don't want big changes. At least they don't want big changes downwards. They are more, they have greater affinity to accept big changes upwards but if you accept big variations up you have to accept big variations down. And then the third element we put into the strategy is that we want to use the freshest piece of information, the new biological information, all the time we will update, all the time [...] Then we made a harvest control rule based on these assumptions where we have used [the precautionary fishing mortality] really as a [desirable] target and not as a [lower] limit value. And that is of course not what you should do but at this historical

time [within the JNRFC] it was what could be achieved – and also because we don't really know what the [optimal] target fishing mortality would be (Directorate of Fisheries representative-a, personal communication, May 2007).

At the core of the HCR for the NEA cod fishery was the three-year averaging process for setting TACs<sup>242</sup>. This implied that the TAC for each year should be the one resulting from the estimation of the average TAC for three years<sup>243</sup>. Every year the estimation of the average of the TAC for three years should be repeated based on the updated information of the NEA cod stock, as follows:

You cannot have the same quota every year, you have to take care to look what's going on in the sea [...] if things are going up you will sort of be taking something out in advance [because the three year average will be higher than the one year TAC], if things are going down you will start putting on the brakes early (Directorate of Fisheries representative-a, personal communication, May 2007).

Yet the TAC for the following year should not fluctuate up or down by more than 10% compared with the previous assessment. This was indeed the measure that should provide stability to the fishing industry – with the 10% figure being somehow 'creative' (Directorate of Fisheries representative-a, personal communication, May 2007).

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<sup>242</sup> The core of the HCR consists of three steps (see ICES, 2004a). First, estimate the average TAC level for the coming 3 years considering the fishing mortality that is consistent with the precautionary approach. The TAC for the next year will be set to this level – starting value for the 3-year period. Second, the year after, the TAC calculation for the next 3 years is repeated based on the updated information about the stock development, however the TAC should not be changed by more than 10% up or down compared with the previous year's TAC. Third, if the spawning stock falls below the levels considered precautionary, the parties should consider a lower TAC than the application of the 10% limit would imply.

<sup>243</sup> Notably, contrary to the HCR for the North Sea flatfish fishery no fishing effort regulation (e.g. days at sea) was introduced. Neither Norway nor Russia would even consider it: "Days-at-sea on top of quotas is a symptom of an inadequate structural policy – too many boats chasing too few fish. In a way, it institutionalises inefficiency in the fishing fleet" (Directorate of Fisheries representative-a, personal communication, May 2007).

The JNRFC sent this HCR to ICES for evaluation in 2004<sup>244</sup>. ICES declared that the rule would not meet the precautionary principle because it was incomplete. The HCR did not describe how the TAC should be calculated if the stock biomass did not amount to the level considered precautionary (ICES, 2004a). In other words, in its original formulation the HCR did not include extraordinary measures such as its own suspension when the biology of the stocks required more radical measures to restore or remain on the safe side. In response to ICES, the JNRFC introduced an exception to the 10% stabilising mechanism. If the fishing mortality fell below the precautionary levels fixed by ICES in any of the three years used to calculate the TAC, there should be no limitations on the year-to-year variations in TAC in order to restore the precautionary biomass levels as soon as possible<sup>245</sup>.

ICES had to decide on the suspension of the HCR whenever the risk for the NEA cod stocks suddenly increased beyond what was considered precautionary (IMR biologist-a, personal communication, May 2007). The authority assigned in principle to ICES was significant, or at least from Norway's side:

There is a wish in the Norwegian side that their work, the estimates should go through ICES and be given a quality stamp on it, that it has been presented by ICES to the international community [...] all the work is done by Norwegian and Russian scientists of course but they meet in ICES and get it reviewed by members from many countries (IMR biologist-b, personal communication, May 2007).

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<sup>244</sup> Notably, “[this] was one of the first attempts to [assess] harvest control rules [in ICES], or at least not far from the first attempt” (PINRO biologist, personal communication, May 2007). Therefore, likewise the case of the evaluation of the HCR for the North Sea flatfish fishery at STECF, this became a pioneering exercise at ICES.

<sup>245</sup> If the spawning stock falls below precautionary levels, the procedure for establishing TAC should be based on a fishing mortality that is proportionally reduced below the fishing mortality considered precautionary (ICES, 2005).

Notably, the Norwegian fishing industry subscribed as well to this idea. In contrast to their EU counterparts in the North Sea, the Norwegian fishing industry was generally supportive of ICES, as follows:

We find that ICES a body that they do very good work and you have international discussions about all these principles and there you have, what is the English word, you have a neutral body [...] of course we have our own scientists in this body but also other; you have scientists from all the countries going through this [peer-reviewing] (NFA representative, personal communication, May 2007).

Therefore, in Norway, ICES was perceived as a suitable scientific arena to invest the HCR with authority. However, the Russian officials were not so keen on this, at least in the view of the Norwegian biologists:

The Russian side [at the JNRFC] said okay, it's our scientists that do the work in ICES, why shouldn't we just keep this bilaterally between Norway and Russia. Is it necessary to go through ICES? That's only double work because all the work is done by our scientists, so maybe it's not necessary. That may be [...] but Norway has [the position] that other [international] parties should be also taken into account [as referees] because Russia is a large country and Norway is a small country (IMR biologist-b, personal communication, May 2007).

Once the HCR was amended in 2004, the JNRFC asked ICES again to verify that the HCR was now precautionary in accordance to the precautionary approach. ICES then spotted a small issue with the wording of the rule<sup>246</sup> and it was all “back and forth

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<sup>246</sup> This was described to me as follows:

If I recall this correctly there was a clerical error or possibility of misunderstanding with regard to [which 3 years to use for the calculation of the TAC] when the HCR was originally drafted. The ICES WG discovered this and asked for an amendment so that the wording of the rule could be in accordance with their interpretation and what they believed were the intention of the parties (Directorate of Fisheries representative-a, personal communication, May 2007).

between 2003 and 2004 in order to fine tune the exact formulation [of the HCR]” (Directorate of Fisheries representative-a, personal communication, May 2007).

The modelling played a role in both evaluations carried out by ICES and, it is important to reflect on it. It was, first of all, a biological evaluation *a priori* of the impact of the HCR on the NEA cod stocks. Therefore, the first thing that was needed was some sort of modelling of the population dynamics of the NEA cod stocks, including issues like ‘cod cannibalism’ and reflecting the uncertainty in the stock-recruitment process. In the case of the evaluation of the NEA cod, the ‘population model’ was one already existing before the evaluation of the HCR known by the name of ‘CodSim’ (IMR biologist-a, personal communication, May 2007). On top of the ‘NEA cod population dynamics model’, the ICES AFWG used another model called PROST<sup>247</sup>, a stochastic simulation to measure the performance of the HCR in the long-term. Other important choices in the evaluation of the HCR were the level of acceptable error due to overestimations in the stock assessment and overfishing to be considered in the exercise (ICES, 2005).

Notably, PROST was a new piece of software written in Java for the evaluation of the HCR NEA cod. This was necessary in order to incorporate the novel 3-year averaging process for setting the TAC (ICES, 2005). Its development became a joint project between Russian biologists at PINRO and their Norwegian counterparts in IMR under the scrutiny of ICES AFWG. IMR led this effort but, “in terms of how the model would be set up, that was a collaboration [between the two groups of biologists]” (IMR modeller, personal communication, May 2007). And the advantage of making a new simulation tool from scratch was that:

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<sup>247</sup> Standing for ‘PROjections STochastic’.

We can do all the negotiations in the small bits and pieces of the model, so we can create a model together [...] [like for instance] do you want to run the model with a time step of one month or one year? [...] There could be more practical things like agreeing on which programming language or framework things should be made in. Yes, that's not necessarily a trivial thing" (IMR biologist-a, personal communication, May 2007).

In essence, the modelling served a role of translation, as pointed out by one of Norwegian biologists directly involved: "Both because their English is not always, it's improved a lot but some of it may be unclear. And it's also a Russian tradition of, well, the Russian language opens for formulating things, tends to formulate things in much more cryptic ways" (IMR biologist-a, personal communication, May 2007).

Based on the consensual projections of PROST, in 2005 ICES finally established that the regulatory rule for the NEA cod fishery was in accordance with the precautionary approach (ICES, 2005). There was now green light for the use of the HCR. And like in the case of the HCR for the North Sea flatfish fishery, the management approach and the HCR were flexible to redefinition in the mid-term:

I would say that this management strategy on cod is a first generation management strategy and harvest control rule. First generation and will obviously be improved in the future or altered or changed. And the most obvious change will be when you come to another [fishing mortality] value than [the fishing mortality considered precautionary by ICES] as the one chosen as the target value future (Directorate of Fisheries representative-a, personal communication, May 2007).

Yet precautionary levels were as far as things could go for the time being. This was clear within the biologists as well:

This isn't an optimal solution but it is optimal in the sense that it led to an agreement between the parties [Norway and Russia]. And in the real world that's a very, very difficult question, that's a difficult task. You cannot just present ideal solutions and then think you can get them [...]



this is the real world, so this is not an academic exercise. It's working for thousands of fishermen in Norway and Russia (IMR biologist-b, personal communication, May 2007).

In this sense, and in contrast to the North Sea flatfish case, the Norwegian policy-makers prioritised the economy of the fishery over the biology, providing the precautionary limits were met: "We are doing what we are doing and the scientists are doing what they are doing to help the fishermen survive as fishermen. I would say we are not Greenpeace [...] We are here to see that the industry makes money but it has to be sustainable [in a precautionary sense]" (Directorate of Fisheries representative-a, personal communication, May 2007). In reciprocity, the Norwegian fishing industry supported the rationale behind the HCR: "Our opinion was that [the HCR] was a necessary instrument to introduce and try to get into that agreement and then we, afterwards, could maybe look further on some of the elements in it" (NFA representative, personal communication, May 2007).

Once the HCR was approved and entered into operation it was expected to make a relevant difference in the decision-making process. According to the Norwegian policy-makers the decision about the TAC for the NEA cod should turn into a more rationalistic process:

So in a sense you have a more mechanised process [of making decisions], a process that in the past was rather more political. Now the [political] is in deciding the strategy and the control group, then the rest is [applying] mathematics [...] You have moved away from who was the best negotiator in the middle of the night or whatever to a system which is foreseeable what is going to happen [with the quotas]. And if you want a higher quota, you have to attack either the data (the input here), or the harvest control rule, or the management strategy and then you can have a more intellectual debate about okay, so you mean, you really mean that [for instance, quota] stability is not worth looking at. So that's your position, okay. And you? And you? You are getting a more enlightened discussion than who says the highest [quota] number (Directorate of Fisheries representative-a, personal communication, May 2007).

Therefore, the idea with the HCR was to establish a more clinical approach to the annual setting of the quotas in order to depoliticise this critical step in the management of the NEA cod fishery. And, as part of this depoliticisation, ICES held the authority to decide on the very application of the rule in situations of risk for the stocks.

All in all, the establishment of the HCR was an uncontroversial exercise that did not challenge the distribution of authority in the existing framing, as it happened in the case of the North Sea flatfish fishery. The modelling involved in the evaluation of the HCR was a joint effort by Norwegian and Russian biologists and helped towards the consensual agreement over the evaluation. Once ICES assessed the HCR as compliant with the precautionary approach in 2004, the instrument was ready to be used for the setting of the quotas. This is what happened indeed in 2005 with the quotas for 2006 and, therefore, one would be initially tempted to conclude that the HCR automatised the management of the NEA cod fishery by virtue of an agreed new linear division of labour between policy-makers, who set up the management objectives and harvest control rules, and scientific advisors, who ‘do the maths’ needed to apply the HCR and come up with the quota figures. Yet, to say that the new sociotechnical framing delivered a depoliticised and mechanised decision-making process was too early an assumption, as I shall explain in the next subsection.

### **7.2.3 How the harvest control rule for the North East Arctic cod fishery worked**

In 2006, it became apparent that the HCR was framed for a different world, one much more compliant than it was in actual fact. Indeed, the HCR became seriously undermined by the Norwegian government’s latest reporting on the substantial scale

of ‘illegal, unreported and unregulated’<sup>248</sup> (IUU) fishing of NEA cod in the Barents Sea, largely carried out by Russian fishermen. According to the Directorate of Fisheries estimations, the amount of this IUU fishing was not only large but also growing exponentially in 2006 (Directorate of Fisheries, 2007). As I will illuminate, this problem had a direct impact on the implementation of the HCR, which became a political issue.

IUU fishing is an issue of lack of observance of the fisheries regulations in place and involves fishing over the quotas. Where it happens it is conducted by ‘modern pirates’, so to speak, who do not report catches by definition. As follows, a high amount of IUU fishing makes any fisheries management approach based on accurate catches figures impracticable<sup>249</sup>. A thorough examination of the causes behind the scale of IUU fishing within the Russian industry – as well as of the Norwegian fishing industry general compliance – falls out the scope of this chapter<sup>250</sup>. Instead, I

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<sup>248</sup> I am using the abbreviation IUU because that is the usual currency but, purely speaking, in the Barents Sea there was no unregulated fishing of NEA cod.

<sup>249</sup> In a sense, when it came to the uncertainty in the catch figures, the consequences of the IUU fishing problem in the Barents Sea with the NEA cod stocks came very close to those of the plaice discards in the North Sea.

<sup>250</sup> Notably, an interviewee considered that IUU fishing in the Barents Sea was mainly a Russian phenomenon for the following reasons:

We share this resource equally. So and we share the other resources also fairly on an equal basis. And that means that after the Soviet Union collapsed, where both countries are basing themselves on the world market prices, it tends to be that the welfare function or whatever coincides. So it should be possible to reach some sort of solution on this issue on the harvest control rule, which took care of both parties’ interests in the same way. Except for one [reason]; there is one [exception] if you do this [analysis] as a bio-economic exercise, the interest rate of Russia, of Russian fishermen – the interest rate, [do] you want to fish one kilo today or two kilos tomorrow and the fishermen tend to have an extremely high interest rate, at least psychologically. From the government side they will, the minister of finance will say that the interest rate you should use in that kind of cost-benefit [analysis] should be 5% or 3% or yeah something in that range. So what you could see as a difference of opinion between Russia and Norway would be that the Russian fishermen would have an extremely high interest rate, they will say ‘no, I want one kilo today. Even if you say we can get thousand kilos tomorrow [because of the growth of the fish], I would still prefer one kilo today’. But that is a mechanism outside the logical one in a sense. Or, I assume, it has something to do [with the fact] that, from the Russian industry they could say ‘but here, if I don’t get that one kilo today, I will not be there tomorrow’ so that means I have from logical reasons an extremely high interest rate. I will not sacrifice one kilo today to get thousand tomorrow

will show, first, how the IUU fishing was conducted and, second, how it was tackled by the Norwegian authorities.

Coming with the first point, the main mechanism used to avoid the control of the Russian and Norwegian authorities was the transshipping of fish in the Barents Sea. Transshipment was a common practice in Soviet times, when the fleet consisted of a large network of trawlers, mother-ships and transport vessels that carried the fish from the open seas into the mainland. Yet at the turn of the 21<sup>st</sup> century the Russian transshipment emerged as a preferred mechanism to smuggle the overfishing of cod and also haddock, transferring these IUU catches from vessel to vessel in the Barents Sea and then landing them in third countries, usually with transport ships under a flag of convenience (Stokke, 2005).

The Norwegian government reacted to the IUU problem with an evidence-based approach. In 2003, concurrent with the approval of the HCR, the Directorate of Fisheries undertook its first serious effort to estimate actual Russian catches in the Barents Sea over the year before. In the following years they continued to estimate unreported catches of NEA cod. The results of these annual estimations were 90,000 tons in 2002, 115,000 tons in 2003, 117,000 tons in 2004 and 166,000 tons in 2005 (Rowe, 2009). During my fieldwork in Norway in 2007 the Norwegian authorities were in the process of estimating the figure for 2006 but so far the trend was clearly upwards. The existing figures already implied between 25-40% of annual overfishing against the NEA cod reported catches (Rowe, 2009).

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because tomorrow I am dead or bankrupted (Directorate of Fisheries representative-a, personal communication, May 2007).

Notably, Hønneland (2004) provides a thorough account of some of these issues.

Although these figures were produced mainly by the Directorate of Fisheries, they were also taken to the ICES AFWG annual meetings so that they could be taken into account in the NEA cod stock assessment process. This indeed opened up a whole discussion in the AFWG, as follows:

The first time we had these data from the Norwegian Directorate of Fisheries [in 2003] they were secret data so we weren't allowed to print out, we had a printout of the information but we were not allowed to copy it or distribute them in any kind of way [...] And there was a big discussion on whether we should include this information or not in the [Arctic Fisheries] Working Group meeting because they [the Russian delegation] didn't know that we were coming with this data. [And indeed] the Russians were shocked because the numbers were from the Norwegian Directorate of Fisheries and were based on the satellite tracking and some coastguard sampling [...] it was a minimum level of illegal catches but still it was 90,000 tons [...] [Then for] the next meeting [in 2004] some of my colleagues [at IMR] had prepared, based on the same kind of data, had prepared some calculations which were more statistical, and was not a 'least estimate' but a real estimate [and IUU] was higher. The year after we included the same kind of calculations on haddock [and] when the haddock data was introduced they opposed it, the Russians [...] So we agreed on [...] lower illegal, lower numbers for the illegal fishing. You have to remember that most of this illegal fishing was Russian so you have the political tension parallel with the Ministers of Fisheries of Russia and Norway having a lot of political meetings etc. trying to solve the problem (IMR biologist-d, personal communication, May 2007).

The issue of IUU catches not only turned out to dominate the discussions at AFWG ICES, but also raised general concerns elsewhere in ICES. An example of this was an *ad hoc* review of the IUU issue for NEA cod and haddock that was carried out in October 2006 by ICES scientists not involved in the AFWG. Notably, they put forward explicit concerns about using the HCR given the IUU problem, as follows:

The ICES advice should take into account any uncertainty about the level of IUU catches. This could be done by carrying out assessments with maximum and minimum estimates rather than selecting a single figure and provide the appropriate catch options related to these. However, this would only show that a fixed harvest control rule does not help to

manage the fisheries exploiting the NEA cod, because the two extremes will result in a range of options to choose from, while there is no scientific argument why one option would be better than the other. If countries are faced with IUU fishing, they should set as their first priority to solve this problem rather than expecting from scientists that they can provide reliable advice (ICES, 2007b:644).

Therefore, the levels of IUU fishing had direct consequences for the implementation of the HCR. The IUU figures produced by the Norwegian authorities and taken up by ICES<sup>251</sup> meant that the fishing mortality in the NEA cod fishery was higher than intended with management plan for stock. Putting it simply, when ICES evaluated whether the HCR complied with the precautionary principle they did not contemplate such a severe scale of IUU fishing<sup>252</sup>. Taking now into account the new starting conditions for the application of the HCR brought by the estimated amounts of IUU fishing, ICES advised in 2006 the suspension of the application of the HCR. The 10% ceiling for inter-annual change in the TAC was too conservative a measure to bring the stock levels back to those considered precautionary. Instead, the TAC for the NEA cod for 2007 was established using the principle of recovery to precautionary levels within the next year that ICES had been employing before the advent of the HCR (ICES, 2006).

At this point, it is important to recall that the idea for the HCR was that it should save policymakers the political trading because they had agreed to stick to the outcome of this rule as applied by ICES. However, this division of labour turned out to be at stake when the JNRFC decided to sidestep ICES advice and apply the HCR in order to establish the NEA cod quotas for 2007:

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<sup>251</sup> Notably, “[t]he Norwegian Directorate of Fisheries increased its efforts to estimate actual Russian catches in the Barents Sea. Based on the results, ICES estimated unreported catches of Northeast Arctic cod” (Rowe, 2009:42).

<sup>252</sup> As one of the biologists pointed out: “When we did the evaluation [IUU] was not so big an issue. That was a couple of years ago” (IMR biologist-a, personal communication, May 2007).

[ICES] abandoned [the HCR] on the reasons that you have decided to make a TAC and made a decision on that, but you haven't controlled [the starting conditions][...] And the Commission did say no, we will leave this rule, ICES has said not to use it but we will use it (IMR biologist-b, personal communication, May 2007).

Notwithstanding their endorsement of the ICES 'quality stamp', the Norwegian government opted for a pragmatic decision in political terms and used the HCR. It helped that in Norway the decision to sidestep ICES advice on this particular occasion enjoyed wide social support by stakeholders. From the fishing industry to environmental NGOs, they all stressed the political advantages of the HCR instrument, as follows:

The advice from ICES was that you must set aside the control harvest rule, you couldn't use the plus-minus 10% [variation in the TAC compared to the previous year] but we disagreed with this and also the Norwegian Directorate of Fisheries, and the mandate from the ministry to the [Norwegian] delegation [at JNRFC] was not to agree with the [ICES] ACFM advice [...] It was very important to continue the discussion with Russia based on the harvest control rule [...] that was the right way to handle this issue last year when we negotiated the quota for 2007 (NFA representative, personal communication, May 2007).

I will strongly, strongly argue that we should keep this harvest control rule. It is the only way we have to keep the political controversy around fisheries management away, we need it because that is how you can lock [the policy-makers] to have the management and not the annual fight, and that's very important (environmental NGO representative, personal communication, May 2007).

Therefore, while according to ICES the HCR was not strong enough biologically, it was considered politically useful in Norway and (presumably) Russia. By and large, this is how the HCR managed to endure as a new addition to the sociotechnical framing for the management of the NEA cod fishery.

All in all, the HCR, as the maximum expression of the sociotechnical framing in place to manage the NEA cod fishery, was not able to depoliticise the decisions on the final quotas; the division of labour anticipated with the implementation of the rule did not necessarily suit the political interests. Already in 2006, it was the policy-makers at the JNRFC, and not ICES as envisaged, who made the ultimate decision of whether the HCR should be applied. Far from depoliticising, the application of the HCR brought politicisation as a response to the problem of IUU fishing.

#### **7.2.4 Reframing the illegal and unreported fishing overflow**

By 2007 the amount of unreported fishing – and therefore, the degree of uncertainty in the catch figures used to support the management of the NEA cod fishery – was the main political priority. Following the 35<sup>th</sup> session of JNRFC in 2006, a special ‘Working Group on Unreported Catch’ had been established with a mandate to analyse data provided by both Norway and Russia on catches and transshipments of NEA cod – as well as haddock. This working group met three times in 2007 in order to come to an agreed assessment (Office of the Auditor General of Norway, 2007).

By and large, the ambition of a more depoliticised decision-making process through the HCR was not feasible in the face of the scale of IUU fishing. All the efforts at the JNRFC focused on reducing the size of this problem:

[IUU fishing] has taken all the time for the Fisheries Commission so discussion on improvement on harvest control rules, getting the haddock control harvest rule, getting that in place, principle discussions on long-term benefits contra short term catches, that has not been possible to take up in the Commission. It has only been on how shall we reduce, how shall we get rid of this IUU catching (IMR scientist-c, personal communication, May 2007).

Yet the Russian fisheries management authorities did find it hard to accept the Norwegian claims that the problem was so severe; especially in 2007 when, for the



first time, the Russians produced their own IUU figures, which they sent to the ICES AFWG annual meeting.

Using a different modelling methodology than the Norwegian authorities, the Russian estimates ended up being very different:

This year [2007] the Russians opposed to the way the cod numbers were produced on illegal fishing [...] The Norwegians tried to estimate how much is taken off record or is taken illegally. Last year [2006] was between 110,000 and 160,000 tons, the Russians say no, no, it was 30,000 tons. So there's quite a disagreement on that (IMR biologist-b, personal communication, May 2007).

Significantly enough, the disagreement in ICES was between IMR and the 'Russian Federal Research Institute of Fisheries and Oceanography' (VNIRO, for its acronym in Russian), the Moscow-based organisation attending the ICES AFWG, inviting controversy<sup>253</sup>: "On the one hand you have the work in PINRO, with a very long

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<sup>253</sup> A 'one-side' snapshot of the controversy goes as follows:

VNIRO says to the world that there is lots and lots more fish in the Barents Sea than PINRO and ICES says. And they are doing this very aggressively and bringing this into the Arctic Fisheries Working Group so it is not easy to work there [...] our feeling is that we have a politics-free arena to work in but it is a little more difficult in the Arctic Fisheries Working Group. I feel the problem is, first of all, that these things are politicised (IMR biologist-e, personal communication, May 2007).

Notably, VNIRO had recently advocated for a new methodology to calculate the size of the stocks that returned much higher numbers of fish. Based solely on catch rate information from the commercial fishing vessels and some gross assumptions, as follows:

[VNIRO scientists] divided the seas into small squares, [and] the assumption that we can't swallow is that within such a square the fishing vessels would be fishing things that are not homogenous[ly distributed]. So the fishing vessels would fish in the areas within that square where the density [of fish] is largest but what they do in their method as far as we've understood it, is that they take catch rates within each of these square and then they multiply that up with the area over the entire square (IMR biologist-a, personal communication, May 2007).

So what was the possible politics behind this move? In the opinion of one of the Norwegian scientists at IMR, Russian authorities did not necessarily orchestrate it: "It's not Russian politics. They do it

tradition of working in ICES, through the ICES working groups, together with us, joint surveys whatever. And on the other hand, you had the Russian institute VNIRO, which has a “different” outlook on things” (IMR biologist-e, personal communication, May 2007).

Notably, it was VNIRO and not PINRO that ‘presented’<sup>254</sup> the Russian IUU figures at the AFWG in May 2007, as follows:

There is no formal ‘Russian delegation’ at such meetings [of the Arctic Fisheries Working Group] [...] I would call the Russian estimate a VNIRO estimate because there wasn’t an estimate by PINRO on that. [Indeed] I think it seemed like the PINRO people were more likely to believe our figures but they were keeping a low profile in this issue [...] the division was between Norway and VNIRO – PINRO and the other countries [at the AFWG] did not enter into this discussion. The IUU calculations were submitted to AFWG by VNIRO scientists. PINRO scientists stayed out of the discussion as far as possible (IMR biologist-a, personal communication, May 2007).

The final report of the ICES AFWG meeting in May 2007 reflected the divergent statistical approaches between Russian and Norwegian authorities, as follows:

Two analyses of potential unreported landings of cod and haddock, provided to ICES by national delegates from Russia and Norway, were made available to the AFWG for consideration. The estimates by Norway for 2006 were derived based on the same methodological approach applied to obtain such estimates for 2002-2005. The Russian analysis provided estimates of potential unreported landings for 2004-2006. The Norwegian method was based on the following: information

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because they feel it’s good [to promote themselves]. They can go to their authorities and say ‘we can give you more fish’, kind of thing. But I personally think it’s not driven by the highest authorities, it has something to do with Russian institutions fighting for their place in the system” (IMR biologist-e, personal communication, May 2007).

<sup>254</sup> It is noteworthy that “the calculations were made not by VNIRO [themselves] but by group of specialists from the fishery control authorities” (PINRO biologist, personal communication, May 2007).

from inspections at sea of fishing and transport vessels in the Norwegian Economic Zone, including species composition of catches and amounts of transhipped fish products, analysis of data on landings in the Norwegian ports and ports of third countries; information on transhipments at sea and VMS [(Vessel Monitoring System)] data from the *Norwegian Economic Zone*. The Russian method used the following: VMS data on operations of fishing and transport vessels in the *Barents and Norwegian Seas*, information on landings in Russian and Norwegian ports and ports of third countries; daily reports by fishing vessels, including on species composition of catches, amounts of transhipped fish products, time of fishing, daily catch rates by vessel type and fishing area [...] The Norwegian method gave considerably higher estimates of unreported catches [...] The AFWG was not able to agree on which of the estimates to use, and found no justification for combining the two estimates in any way (ICES, 2007b:3, my emphasis).

Averaging the two estimates would have made no sense since the divergent estimates were rooted in different world-views. At the centre of the controversy in the ICES AFWG meeting in May 2007 was the disagreement on the species composition of the transhipped catches. The Norwegian IUU estimates, based on the Norwegian Exclusive Economic Zone only, excluded the vessels carrying species other than cod and haddock – for instance, pelagic fish or king crab – and also vessels where there was no information about the species composition. The Russian estimates were based on information on the species composition of the total catches in the Norwegian and Barents Sea. More than half of this total was estimated to be other species – polar cod, king crab, herring, blue whiting, mackerel, redfish – and the Russian estimates assumed that this reflected the species composition within the transhipped vessels. The Norwegians considered this to be unscientific, arguing that the species composition of the catches in the Norwegian Sea had very little relevance to the species composition of transhipped fish in the Barents Sea (IMR biologist-a, personal communication, May 2007).

By and large, the two different approaches at the ICES AFWG meeting led to a redistribution of authority over what should be the scientists' role and what the policy-makers' role:

AFWG concluded that IUU catching problem (including discrepancies of methods) should be resolved by fisheries authorities. There is a Permanent Russian-Norwegian Committee on fishery control. It is a part of JRNC [Joint Norwegian-Russian Fishery Commission]. They have particular meetings on resolving this problem [...] In my opinion, in general there was no clear controversy on IUU like: ‘which one is more accurate?’ There are so many unknown details in both. There was information by each vessel, fishery and transport, considered in those calculations. We scientists simply do not have this information “on the table” to decide. It is a business of appropriate fishery control authorities [...] It is rather politically sensitive problem. The Permanent Committee on Control of JRNC is an appropriate place to do such a job and give the best estimates (PINRO biologist, personal communication, May 2007).

In other words, the ICES biologists were not comfortable with the blurring of the political and the advisory arenas. In re-establishing the linear division of labour with the JNFRC authorities, they envisaged themselves simply taking up the IUU figures from the policy-makers and using them to evaluate whether the HCR could be applied or not. This is the way things went indeed in 2007:

In 2006 when ICES ACFM was giving advice and said this harvest control rule for cod doesn’t correspond to precautionary approach because such a high level of implementation error, high level of illegal fishery [...] But this year we [the ICES AFWG] tried to evaluate directly in our report, in the cod section, the different level of implementation error and tried to establish the influence on the results and we found out that the evaluation implementation error is less than 30%, and if the implementation error is not higher than 30% [then the HCR] will be still in accordance with precautionary approach (PINRO biologist, personal communication, May 2007).

Therefore, this time the uncertainty in the catch figures introduced by the IUU fishing problem did not undermine the precautionary goal of the harvest control rule according to ICES. In other words, in 2007 there were no scientific arguments in ICES advising not to apply the rule and the TAC advice for 2008 was indeed set accordingly at ICES ACFM. Yet the NEA cod quota for 2008 was set by the JNRFRC at a slightly higher level than what ICES ACFM had advised using the HCR – the NEA cod quota for 2008 was 430,000 tons, an extra 21,000 tons with respect to

ICES advice. As it had happened the year before, the HCR did not depoliticise the decision about the quotas as originally envisaged; it was ultimately the policy-makers and not ICES biologists who exercised the authority on when or how strongly to apply the HCR to establish the quota for the NEA cod, as follows:

The Norwegian estimate for 2006 IUU catches was 117,000 tons and was produced by Norwegian Directorate of Fisheries in February 2007 and used by the Arctic Fisheries WG and ACFM in their assessment and advice [in May 2007]. This estimate was a mean value. During the latter part of 2007 there was however, an extensive process with our Russian colleagues trying to improve the database for the estimation and trying to agree on a common estimate. We did not fully succeed, but as a result of the exchange of detailed data on a trip-by-trip basis we, on the Norwegian side, were convinced that 117,000 tons was an overestimate that should be reduced at least to 97,000 tons, maybe even further down to a minimum estimate of 63,000 tons. Based on our [current] observations in 2007, including the introduction of port state control from the first of May 2007, we were also convinced that IUU fishing of cod was further reduced in 2007 [...] at last it seems that we [the Norwegian Directorate of Fisheries] may be successful [in reducing the problem]. The scientists [at the following JNRFC annual meeting in October 2007 in St. Petersburg] were asked to reassess the stock and the advice taking into account the new and improved information with regard to IUU-fishing. This recalculation gave as a result that the TAC, in compliance with the established HCR, could be increased with 21,000 tons compared to the [ICES] ACFM advice. This increased quota was shared between the parties [at the JNRFC] in the traditional way [50-50] (Directorate of Fisheries representative-a, personal communication, February 2008).

This happened again the following year. In 2008 the JNRFC authorities used the same reasoning to set a quota for 2009 of 525,000 tons while the HCR advised a TAC of 473,000 tons (IMR, 2009). As the argument goes, the setting of the quotas above the ICES advice on the basis of the HCR became a way to show that fighting IUU fishing could bring larger quotas to the Norwegian and Russian fishermen. Notably, this contravention of the HCR raised some eyebrows in ICES, to the point that the risk to conservation for the NEA cod stocks was raised again, as follows: “[ICES] presumed that the plan [HCR] should be strictly followed for setting

TAC[s], and the deviation from the management plan in last year is not considered to be a precautionary practice” (ICES, 2009:17). Nonetheless, for the Norwegian government setting aside the ICES advice for the sake of fighting IUU fishing was paying off and, overall, was perceived as a sustainable practice:

The results of the regional Russian and Norwegian work have been very encouraging, producing a reduction of IUU fishing of cod in the Barents Sea by 84% from 2005 till 2008. At present, this trend of reduced overfishing seems to be strengthened even more [...] This represents a remarkable example of how concerted action against IUU fishing can indeed be successful. Russia and Norway will be vigilant and act in a pro-active way against any signs of IUU fishing of the cod stocks in the future. Consumers can therefore be reassured that fish from the Barents Sea originates from sustainable fisheries (The Norwegian Ministry of Fisheries and Coastal Affairs, 2011:1).

All in all, the evidence presented illuminates that how to apply the HCR was a political decision in the presence of an overflow, that of IUU fishing, which had to be tamed first. Or bluntly put, the HCR opened up a space for political work in the light of IUU fishing. In this sense, even the ICES AFWG itself turned into part of the political arena at certain stages of the policy process, which the biologists had to sort out by conducting boundary-work<sup>255</sup>. I shall offer some conclusions below but first I will come back to the main case study and look at the implementation of the HCR in the light of the fragility just exposed in the case of the Barents Sea NEA cod fishery.

### ***7.3 A tailpiece for the North Sea flatfish fishery case study: The implementation of the harvest control rule for plaice and sole***

With the case of the HCR for the NEA cod fishery as a backdrop, it is time to account for what happened with the HCR for the flatfish fishery in the North Sea immediately after it was officially approved in July 2007. How did it work for

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<sup>255</sup> As defined and understood in STS, boundary-work is a particular kind of political work.

decision-making? How much of a difference did it make for the management of the North Sea flatfish fishery? As illuminated by the NEA cod fishery case study, HCRs may bring more of the same political trading rather than actual depoliticisation of the decision-making process. In this section, I shall show how the case of the HCR for the North Sea flatfish fishery is consistent with this pattern.

The Council of Ministers adopted the long-term management plan for North Sea flatfish fishery in June 2007 – for implementation as of 1 January 2008 (Council of the European Union, 2007). In the mean time, in October 2007, the ICES ACFM had to adopt the TAC advice for 2008. In contrast with the expectations of the NSRAC fishing industry, at that point ICES did not use the new HCR for the plaice and sole stocks. Instead, ICES continued to provide the advice in the same way it had been doing it over the years, that is, giving priority to the conservation of the stocks within precautionary levels over the steadiness of the TACs in between years. The industry stakeholders within the NSRAC found the overlooking of the HCR overtly distressing, as follows:

To our great astonishment, the advice on North Sea plaice and sole was not given in accordance with the long term management plan agreed upon by Council [of Ministers]. We were informed that it was not in line with ICES policy to base the North Sea plaice and sole advice upon a plan that had not been reviewed and assessed by ICES. It would appear that ICES has not been requested by the Commission to review the management plan, and that it does not feel any obligation to do so until it receives a formal request from the Commission [...] The development of the long term management plan for North Sea plaice and sole has been a difficult process. Although NSRAC is very aware of the serious short term economic consequences for the fleets involved, we were pleased to see that the agreed plan with its harvest control rules includes many key elements of the advice from the NSRAC. In this context, we find the current advice situation very disturbing. It may signal to the fleets and other stakeholders (including Norway) that ICES does [not] consider the long-term management plan to be sound or appropriate (NSRAC, 2007e:1).

If ICES had used the agreed long-term management plan for 2008, the advice on plaice – the stock in worse shape – would have been 49,000 tons, rather than the precautionary figure of 35,000 tons. In this light, the NSRAC was concerned with the fact that the fishermen could think there was little point in working closely with DG-FISH in the preparation of the plan (NSRAC 2007a).

In response to the situation created, DG-FISH tried to add light to the issue stressing the full independence of ICES at the time of deciding if they agreed with the flatfish long-term management plan officially approved and when to do so. DG-FISH had asked STECF for an evaluation of the HCR in the first place because of the demands for a socio-economic element in the assessment, but STECF was not an independent provider of biological advice (NSRAC, 2007b). Meanwhile, ICES had a standing order to evaluate biologically this HCR – as well as any other HCR – according to the Memorandum of Understanding signed with the Commission and, therefore, no specific request was needed. However, DG-FISH acknowledged that the long-term management plan had been only agreed in summer 2007, while the main ICES advice had been given in May; thus, ICES had time until May 2008 to conduct the evaluation (DG-FISH, 2007a).

The NSRAC industry stakeholders believed that the decision of ICES of not using the HCR could also send what they considered as dire signals to an awaiting Norwegian government (NSRAC, 2007e). This was in relation to the fact that Norway and the EU share the management of some of the stocks in the North Sea. Every November there is a new round of Norway-EU negotiations to decide on common stocks. These include the North Sea plaice stocks, of which Norway receives a small part of the total quota – around 6% only<sup>256</sup>. Norway had already

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<sup>256</sup> It is noteworthy to see what happens with that portion of the quota:



agreed with DG-FISH two or three years earlier that some kind of recovery plan for plaice was needed to be established (NSRAC industry representative-a, personal communication, April 2007), with the responsibility for the crafting of the plan relying on the Commission (DG-FISH conservation unit representative-a, personal communication, March 2007). The long-term management plan was meant to be the step forward that Norway was waiting for.

The concerns of the NSRAC proved not to guess right though. Norway had a critical opinion over the flatfish management plan anyway. At the negotiation table between Norway and the EU in the autumn of 2007 Norway did not accept the terms of the long-term management plan for the flatfish. They argued that the management plan established by the European Commission was too much focused on sole. In their view, the plaice stocks were treated as a secondary species (Directorate of Fisheries representative-b, personal communication, February 2008). By-catch of plaice would continue to be large and, for the Norwegian government, discards were not an acceptable practice – discards were indeed banned in the Norwegian EEZ – and the EU was not doing enough about them:

EU has a plan for plaice and sole combined whereas only plaice is a joint stock between EU and Norway, Norway being the junior partner with a negotiated 7% share of the plaice stock. Our problem with the plan is that it is too much tailored at maximizing the benefits from sole at the expense of plaice. Plaice is very much an unavoidable by-catch when targeting sole, and is discarded to a large extent under the present management regime, sometimes in huge quantities. We think that a viable management plan for flatfish must address this issue (Directorate of Fisheries representative-a, personal communication, February 2008).

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It's a very, it's a small fishery for Norway in the North Sea but of course it's a big fishery in the Netherlands, also of course in Denmark. So we have a share of the plaice quota, plaice stock in the North Sea and we have, if we not fish that quota ourselves then we give it to EU and then get some other more interesting species back. That's the way we have done it (NFA representative, personal communication, May 2007).

In this sense, there was no sign in the new long-term plan of the will to change the technical regulations to minimise by-catch, i.e. increasing the mesh size of the 80mm long beam-trawlers, a perennial request from Norway (Directorate of Fisheries representative-a, personal communication, February 2008). The Norwegian authorities asked to the European Commission to follow the ICES advice that aimed to restore the plaice stocks back to a precautionary level. As for the long-term management plan, “[t]he [two] Delegations agreed to continue the work on developing this long-term management plan and to consult no later than April 2008” (The Norwegian Ministry of Fisheries and Coastal Affairs, 2007:2).

Yet, in the end, Norway opened the door to agreements on the TAC for plaice on an *ad hoc* basis (Directorate of Fisheries representative-a, personal communication, February 2008). In this respect, Norway agreed with the Commission on the plaice TAC for 2008 that emerged from the application of the HCR approved in July 2007, which was significantly higher than the one that had been advised by ICES. At the end of the day, by making the EU fight for a higher TAC than the ICES ACFM advice, Norway could achieve concessions somewhere else<sup>257</sup> in the course of the Norway-EU negotiations (NSRAC, 2007b).

In the end, it was indeed disregarding ICES advice and embracing the HCR that the TACs and quotas for the North Sea plaice and sole were set. According to the HCR, the plaice TAC for 2008 should only go down 2.5%, from 50,300 tons to 49,000 tons, and on 19 December 2007, the Fisheries Ministers of the European Council decided to establish precisely that amount as the North Sea plaice quota for 2008. Meanwhile, in the case of the sole the TAC had to be reduced by 15%, from 14,970 tons to 12,800 tons following the HCR and, the European Council of Ministers

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<sup>257</sup> The EU and Norway share other species in the North Sea such as mackerel, herring, saithe, haddock or the North Sea cod.

decided as well to set the sole quota for 2008 at that level (Dutch Fish Product Board, 2008).

All in all, while the HCR was meant to depoliticise the setting of the quotas, that is, to cut down political trading, on the first opportunity to do the job, applying the HCR turned into a political decision. As follows, it did not make much of a difference with regard to how decision-making was conducted prior to the adoption of the HCR. The parallelism with the case of the HCR for the NEA cod fishery is significant. As in the NEA cod fishery case study, the authority of ICES at the time of evaluating the HCR was overlooked in favour of political interests. When to apply and how firmly to enforce the rule were underdetermined by the scientific authority and subjected to political interests. As the argument goes, the mechanistic division of labour between science and policy that inspired the introduction of the HCR did not see the light of the day. Instead, what the development and deployment of the HCR delivered was another space for political work.

#### **7.4 Conclusions**

Harvest control rules are the third element of the troika of instruments in the analysis of the building of a new framing for EU fisheries management decisions. In this chapter I have examined how the implementation of HCR for the North Sea flatfish fishery worked. Yet, in order to make my claims more robust, I have looked as well into the case of the implementation of the HCR for the NEA cod fishery. I will wrap up the insights from placing the two case studies next to each other in two sets.

The first set of conclusions is on the shifting roles of the calculations observed as part of the reframing efforts of the IUU overflow. In describing the implementation of the HCR for the NEA cod fishery I have portrayed the changing role of the estimations of IUU fishing. The first estimations conducted initially in ‘secret’ by the Norwegian authorities worked as ‘eye-openers’, helping to get an idea of what it

seemed was an escalating problem and to place the issue on the political agenda. Then, when the Russian biologists from VNIRO came about with alternative IUU figures in 2007 in the context of the ICES AFWG and the JNRFC, the estimations served to portray two arguments in dissent. Finally, the latest use of IUU estimates delivered by and for the fisheries authorities aimed at showing that IUU fishing could be curbed. By and large, the role of the IUU estimations shifted as the context evolved and the basis on how these estimations were assessed changed alongside. Notably, in the end the IUU estimations were evaluated for their political usefulness as well as for their representational accuracy.

Second, the HCRs did not work as originally conceived to depoliticise and automatise the decision-making process. The HCR for NEA cod fishery was designed without taking enough into account the increasing scale of IUU fishing that had emerged in the fishery – an overflow (Callon, 1998) to the very act of establishing less than generous fishing rights. Meanwhile, the HCR for the North Sea flatfish fishery was illuminated without involving Norway – a stakeholder in the process – and evaluated without the direct involvement of ICES as a certifying agency. Thus, one can say that the policy processes leading to the two HCRs produced overflows; or in other words, depoliticisation produced politicisation.

Indeed, the implementation of HCRs triggered boundary-work (Gieryn, 1983, 1995) – that is, political work driving the mutual adjustments between science and non-science. In the case of the NEA cod fishery, for instance, the ICES AFWG turned into part of the political arena and the biologists tried boundary-work to re-accommodate a linear or mechanistic division of labour with the political authorities at the JNRFC. Yet this division of labour was repeatedly disrupted in the decisions over when to apply and how strongly to enforce it. And a similar observation can be made for the case of the HCR for North Sea flatfish fishery. Undermining the boundary, DG-FISH overlooked the certifying authority of ICES before applying the HCR in response to the political work by the fishing industry. Norway also agreed in

the end to ignore ICES advice and used the implementation of the ‘uncertified’ HCR for the North Sea flatfish to strengthen its negotiating position with the European Commission on other stocks. Putting it in terms of interactionist co-production, applying the HCRs was not simply calculating the TACs for the following year but also confronting who-had-really-authority-when. Implementation involved social learning, with actors – beginning with policy-makers – domesticating the new space for political work that the HCRs introduced. The HCRs might have come about in response to the problems with the heavily politicised decisions based on ICES annual stock assessments but, at the time of research, all that they offered was some kind of *a déjà vu*<sup>258</sup>.

I have now completed the account of the troika of instruments – integrated assessment modelling, integrated impact assessment and harvest control rules – that supported the building of a new framing for the depoliticisation of EU fisheries decisions, following the regulatory reform in 2002. It was clear that there was still work to do in the establishment of this new sociotechnical framing. As I shall argue, EU fisheries policy is indeed always in the making. It will be already in the next chapter, where I will complete the analysis with the conclusions to this thesis.

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<sup>258</sup> See DG-MARE (2012) for a recent update on the management of the North Sea flatfish stocks and further evidence that progress is at best incremental and very slow. It was only the end of 2012 that both the plaice and sole stocks were considered firmly at precautionary levels, so the CFP reform of 2002 managed to bring the stocks to where they should have been without the reform if the precautionary approach would have worked. Furthermore, the text offers another instance of the states of exception that characterise EU fisheries management. The two-step management plan approved in 2007 has already become an ‘interim’ strategy before reaching the second step. There will be a ‘hot swap’ somewhere in 2014 for a new multi-species management plan under the new Common Fisheries Policy – operational as of 1 of January 2014.

## **8 Conclusions: Sociotechnical framings, governable fisheries, decentred modelling**

### **8.1 Introduction**

In a way, it is easier to highlight the distinctive character of this thesis by stating first what it is trying to move away from. The previous chapters illustrate a research journey. I started to make the first steps towards this thesis with an assumption in mind, which later became a research question in its own right. I set myself to follow the practice of computer modelling for decision-making around in the belief that models and simulations were the central tools upon which the so-called evidence-based policy process relied for their compelling representational capacity. In short, probably influenced at that early point by the visions that scientists tend to ascribe to their models, I wondered whether ‘good models’ for policy were those assessed with better properties to reflect the outer complexities; how did these sophisticated tools get validated in the first place; how to balance simplicity and complexity for representational purposes; or how much simplicity or complexity was enough not just for modellers, but also for stakeholders and policy-makers outside the modelling circles.

However, in the course of several rounds of revisiting the empirical evidence, conducting additional analysis, and reviewing further the literature, I recast the research inquiries with a more pragmatic flavour. Drawing on the cross-fertilisation of the social studies of fisheries and the social studies of finance (Holm, 2007; Holm and Nielsen, 2004, 2007; MacKenzie, 2003; Millo and MacKenzie, 2009), I have addressed three related issues. First, how modelling matters for policy-making is in the ultimate sense a question of being regarded ‘useful enough’ for the policy context. A model will not be written off because of its uncertainties if it is seen as serviceable to policy-making. In other words, the organisational-political usefulness for a given sociotechnical framing can make the model prevail even when the model’s limitations for producing accurate representations are overtly acknowledged. As it has been shown in other contexts (see MacKenzie, 1990), accuracy and

organisational-political usefulness are, by and large, co-produced within a given sociotechnical framing<sup>259</sup>. Second, but closely related, this thesis shows how the usefulness of modelling for policy-making can be understood as providing spaces for political work by policy-makers, stakeholders and scientists. This is indeed how all the different technologies of government that I analysed worked. Third, I argue that modelling does not generally stand alone as a central element of the policy process and the analyst should beware of this common narrative bias<sup>260</sup>.

The practical way to avoid the temptation of giving the models too much centrality is to take the sociotechnical infrastructure that may enable the governing process under certain conditions as the unit of analysis. Policy-oriented computer modelling does not work in isolation. Together with other elements, it is part of a sociotechnical framing (Callon, 1998, 2004; for the similar idiom ‘sociotechnical ensemble’ see also Bijker, 1995) that may facilitate decisions. For the main case study of the North Sea flatfish fishery these elements were the other innovations introduced in 2002 to achieve the depoliticisation of the fish quotas in the EU: long-term management plans based on harvest control rules and *ex-ante* integrated impact assessments of the HCRs. The supplementary case study, centred on the Barents Sea NEA cod fishery, also presented elements of a similar framing exercise – albeit without the idiosyncrasy of Brussels and its Better Regulation strategy.

In order to understand what goes on inside the policy process I have drawn on the co-production framework (Jasanoff, 2004, 2010b; Jasanoff and Wynne, 1998; Lidskog and Sundqvist, 2011; Tuinstra, 2008; Tuinstra et al., 2006). On the one hand, the

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<sup>259</sup> And, as the Barents Sea case study illuminates, sociotechnical framings can be considerably exposed to overflows in fisheries, often a ‘heated’ (after Callon, 1998) context. See for instance Finlayson (1994) for the case of the collapse of the Newfoundland cod stocks, which meant as well the breakdown of the co-production of representational validity and political usefulness regarding the modelling approach used in the management of that fishery.

<sup>260</sup> To recall, Stewart and Williams use the expression to refer to those accounts in which certain elements and issues are given weight (or not) to “augment the perceived social significance and centrality of the locales and actors under study” (2005:24).

constitutive co-production framework highlights the process of ordering the natural and the social to enable decisions, that is, the constitution of the manageable fish and fishermen. This is the process of building a sociotechnical framing. On the other hand, the interactionist strand of the co-production approach does not predetermine any relationship between the framing efforts and the policy process. It encourages the analyst to look into the fine-grained details of the political work that takes place around salient new knowledge practices confronted with existing structures of authority. This political work points at the changing divisions of labour, at the shifting of actors' roles (Lidskog and Sundqvist, 2011; see also Halfman and Hoppe, 2005). As follows, I have illuminated how the instruments that were intended for depoliticisation worked in practice as new polities in the business of making decisions about fish quotas in the EU. Depoliticisation and politicisation were produced together in tension. I shall argue that this perennial tension between depoliticisation and politicisation – framing and overflowing in the language of Callon (1998) – is key to understanding how EU fisheries are made governable.

The structure of the chapter is as follows. In §8.2 I will offer some conclusions over the troika of policy instruments that I have taken as holding together the framing to depoliticise EU fisheries policy following the reforms in 2002. The bottom line was that these instruments did not work as expected and did not bring depoliticisation to decisions about the North Sea mixed flatfish fishery. In this respect, first, I will address how integrated assessment modelling worked in practice in the policy process. Second, I will look at how the integrated impact assessment actually delivered and conceptualise it on the basis of the co-production framework. Third, I will address how the practical implementation of harvest control rules worked in the two case studies. Taking the analysis at the level of the policy process with the troika of instruments that support the framing of decisions as the unit of inquiry has allowed me to arrive at other tangential insights – what I refer to as the by-catch of my research journey. The weightiest one, deserving a section of its own (§8.3), has to do with how are EU fisheries made governable. Finally, in §8.4 I will come back to



the main research question and offer a concluding remark on the appropriateness of decentring of modelling for policy.

## **8.2 Insights into the building of a new sociotechnical framing for EU fisheries management after the reform of the CFP in 2002**

First of all, let us recall the narrative lines of the main case study briefly. To tackle the role of modelling for the evidence-based policy process – in other words, the problem of how to depoliticise policy decisions with modelling – I have mainly focused on the case of a computer modelling-intensive area of public policy in Europe, namely fisheries management at the EU level. This is one of the very few areas governed directly from Brussels by virtue of the EU Treaties, which grant authority to the EU institutions and, in particular to the Council of Ministers, to make decisions over the fisheries resources in the waters under the exclusive economic zone of the European Union.

Some commentators claim that fisheries management is probably the most science-dependent sector in the EU (Daw and Gray, 2005). Before the reform of 2002, the Common Fisheries Policy was certainly a massive top-down mechanistic endeavour with almost no par. The attempt to govern fisheries on the European scale in a centralised and hierarchical style of management demanded a vast science-based ‘knowledge infrastructure’ (after Edwards, 2010) under the control of the European Commission in Brussels. Holm and Nielsen (2004) coined the term TAC Machine to make explicit reference to the core of this infrastructure serving EU fisheries policy. Until its reform in 2002, one could equate without problem the CFP to the annual iteration of the TAC Machine dividing the EU fish stocks into quotas.

Yet, despite all the science – or because of this some would say – the EU has not performed as a successful fisheries manager (Cardinale and Svedäng, 2008; Daw and Gray, 2005; Wilson, 2009). The reform of the CFP in 2002 added more instruments

on top of the TAC Machine so that the process of mechanisation and depoliticisation of EU decisions about fisheries could finally take off, this time to meet a reformulated goal of biological and socio-economic sustainability. In order to distil out short-term political interests<sup>261</sup> from quota decisions, the TACs should now be aligned with a clear long-term management strategy for each fishery passed in good governance style – i.e. with the participation of stakeholders from the RACs in the advice to policy and the implementation of the European Commission's Better Regulation strategy that mandated the anticipation of impacts of all policy alternatives. In addition, the adoption of the scientific concept of MSY following the signature of the agreements of the WSSD 2002 in Johannesburg, enabled DG-FISH to eye the political ambition of having sustainable fisheries no later than the year of 2015 by means of implementing these long-term management plans. All in all, the CFP reform of 2002 was not about replacing the TAC Machine but of 'reframing' (Callon, 1998) it with more (integrated) science and more actors to produce and legitimate this science.

However, the three instruments introduced as part of the reframing strategy to fulfil the long-term management plan for the mixed flatfish fishery in the North Sea – considered somehow a 'laboratory' – did not work in a mechanistic style. On the evidence of the empirical material, these instruments did not manage to bracket off political considerations. It follows from the tension between depoliticisation and politicisation that the integrated assessment modelling, the integrated impact assessment and harvest control rules became "element[s] in political activity" (Shapin and Schaeffer, 1985:332; see also Jasanoff, 2004), that is, instruments for political work, political technologies in effect (Barry, 2002; Lascoumes and Le Galès, 2007). They 'opened up' (after Stirling, 2008) spaces for social learning and reassignment of authority in order to craft new divisions of labour between actors.

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<sup>261</sup> Normally informed by the immediate economic concerns of the national fishing fleets.

The nuances and ramifications of this argument for each of the three instruments follow below.

### **8.2.1 The role of integrated assessment modelling**

In the empirical chapters I focused extensively on modelling practice in response to its ubiquity in the policy process. Integrated assessment modelling is nowadays a method of choice to provide evidence for decision-making in many areas of public policy, but integrated assessment computer modelling does not easily come to the rescue of rational policy-makers. The difficulty with these models is not that they are inaccurate and fallible – that is taken for granted to a certain degree since they are not intended as predictions, just heuristic – but how can they still work usefully for policy despite the fact that in order to mirror the complexity of the real world they have to span across scientific disciplines and beyond, involving actors from different constituencies.

Some STS commentators on the role of computer modelling have approached this question by illuminating how computer models can be useful as a ‘focal point’ for communication and organisational purposes and work in practice as boundary objects, mediators or ‘social glue’ (Edwards, 2001, 1996a; Shackley and Gough, 2002; Halffman, 2003; Millo and MacKenzie, 2007, 2009; Shackley, 1997; Van Daalen et al., 2002; Van Egmond and Bal, 2011; Van Egmond and Zeiss, 2010). As follows, the influence of modelling in policy in such cases is by no means direct but mediated through the coordination of social worlds that are somehow involved in the policy development – I shall come back to this point in the last section of the chapter.

The notion of boundary object has been the most conceptually advanced and popular of the different terms just mentioned above. Following Star (2010; Star and Griesemer, 1989), boundary objects refer to ‘spaces’ where different social worlds

can communicate, interact, reconcile, collaborate, negotiate and, at the same time, remain unchanged as distinctive and recognisable social groups<sup>262</sup>. Modelling practices can offer such spaces for actors to come together and achieve a certain level of coordination despite their differences, so that action – e.g. the policy process – is taken forward. Scholars stress the organisational usefulness of these practices over their capacity to produce real-looking representations (see Edwards, 1996a; MacKenzie, 2003; Millo and MacKenzie, 2007, 2009; Van Egmond and Zeiss, 2010).

At the same time, another relevant discussion in the literature of boundary objects comes from a line of criticism put forward by Star (2010). She observes that most of the studies of boundary objects show a narrative bias towards the ‘object’ at the expense of the infrastructure that the object is part of. Often the analysts of boundary objects have gone along singling out objects that enable interpretative flexibility across a social diversity – the result of this being a ‘hyperinflation’ of boundary objects in the literature. A lot of the scholarly work reads as if potential boundary objects could be context-independently validated against the sociological attribute of ‘plastic enough, yet robust enough’. As follows, many users of the notion miss out the entire organic infrastructure through which social groups cooperate as a result of information and organisational needs (Star, 2010).

Star’s (2010) critical remarks may apply to those accounts where computer models are taken as boundary objects. Let us look in this light at the role of integrated assessment modelling to conduct fisheries management strategy evaluations in the main case study. IAMs are not intended for forecasting but for comparison of policy

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<sup>262</sup> Notably, some scholars observe that the concept can serve retrospective analysis of contexts with stable social worlds – in Star and Griesemer’s (1989) it took 30 years for the boundary objects to develop – but it does not suit equally well the analysis of processes of stabilisation of facts (Fujimura, 1992). In the latter case, the very much still in the making boundary objects can also introduce changes to the social worlds (Van Egmond and Zeiss, 2010).

scenarios. In principle, lack of predictive accuracy should not be an issue. In practice, some scholars suggest that the attributes of IAMs – e.g. they are ambitiously comprehensive and involve too much guessing – make them inherently fragile as boundary objects, leading often to politicisation instead of coordination (Shackley and Gough, 2002). Yet, the attempt to scrutinise the properties of integrated assessment modelling against those of boundary objects is misleading for the reasons exposed above. How IAMs work is, by and large, a contextual and infrastructure-mediated outcome, as this thesis informs by means of the co-production framework.

Drawing on the empirical chapters, evidence shows how the introduction of integrated assessment modelling worked as a space where different actors underwent social learning and revisited their relationships of authority. In particular, the constitutive co-production of the linear relationship linking fishing effort and fishing mortality for the mixed flatfish fishery was a remarkable interdisciplinary outcome of the mutual adjustment between economists and biologists over who-had-authority-when in the advisory arena.

The integration of bio-economic knowledge in the production of model-based scientific advice was a challenge for several reasons<sup>263</sup>. First, because of the institutionalised linear division of labour between biologists and economists, which informed the organisation of the first meeting of the IIA exercise – this changed significantly in the following meeting. Second, there were epistemological odds against the idea of knowledge integration, following the ‘certainty trough’ pattern<sup>264</sup>

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<sup>263</sup> To recall, computer modelling is a suitable territory to critically assess the limits of the integrated approach to the production of evidence, as explored by Shackley and Wynne (1995b) for the case of climate change advice and Gough et al. (1998) for the case of the trans-boundary air pollution.

<sup>264</sup> The ‘certainty trough’ phenomenon described by MacKenzie (1990) is another way to characterise the linear division of labour between the biologists and the economists. Shackley and Wynne (1995b) did this to describe the once one-way hierarchical relationship between climate change modellers and climate change impact modellers, who used to work detached from each other and, in so doing,

(MacKenzie, 1990; Shackley and Wynne, 1995b). Basically, the more IIA exercise advanced, the more the underestimations over the degree of uncertainty across the disciplinary boundaries became exposed. Third, methodologically speaking, (ICES) biologists from IMARES and EIAA economists had very different exemplary solutions for the same problem. And in this sense, biologists and economists were not so much engaged in the production of integrated knowledge as in the shaping of the new advisory arena with their exemplary solutions and tunnelled world-views (see Degnbol et al., 2006). It was hard work overall, and the integration of knowledge observed with the linear relationship between fishing mortality and fishing effort came only after the establishment of a trading zone<sup>265</sup> (Galison, 1996, 1997) towards the end of the second meeting.

By and large, this trading zone had an important significance. Both biologists and economists in the second meeting of the IIA assessed the accuracy of the modelling in conjunction with the clear need to redefine their so far linear and mechanistic division of labour. It was in that context that the trading zone emerged and the IAM, and more precisely the linear relationship between fishing effort and fishing mortality, became considered ‘accurate enough’ and ‘useful’ for the political work of establishing a more organic division of labour for the biologists and economists.

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impact modellers tended to attribute much more certainty to the climate change models than the modellers themselves. The presumption of certainty curtailed questions or feedback to the climate change modellers and favoured instead a linear flow of knowledge.

<sup>265</sup> One could be tempted to identify the echoes of ‘mode-2 knowledge production’ in terms of interdisciplinary and a policy-problem driven context, as opposed to blue-sky academic research organised within disciplinary confines that characterises ‘mode-1’ (Gibbons et al., 1994; Nowotny et al., 2001). However, one must question how much of a change adds up for a new form of knowledge production to be considered as such. Following Rip (2002), mode-1 and mode-2 should be approached as end-of-the-spectrum organisational arrangements for a knowledge production that is always locally and historically situated (Rip, 2002). In this sense, the empirical evidence points at very localised signs of apparent mode-2 in a context where the imprints of mode-1 still shape in practice aspects of the production of knowledge (Carter, 2013a; see also Holm, 2003). This is why the trading zone seems an adequate metaphor.

To reinforce this contextual argument let us look as well at the evidence that emerged when the NSRAC industry representatives adopted the IMARES model as an ‘empirical friend’ (after Yearley, 1991, 1992) that enabled them to open everybody’s eyes to an alternative policy option. Notably, NSRAC industry representatives did not engage in any scrutiny of this model despite two conditions that could have encouraged a sort of post-normal science pattern of interaction: the model’s shaky foundations and fishing industry’s high stakes in the IIA exercise<sup>266</sup>. This regardless as well of the fact that, in the stock assessment process, the NSRAC fishing industry representatives<sup>267</sup> were genuinely capable of engaging with the inner elements of the modelling as ‘external peer reviewers’<sup>268</sup> (see Dahinden et al., 2000, 2003; Darier et al., 1999; Siebenhuner and Barth, 2005; Van der Sluijs et al., 2005; Van der Sluijs, 2002b; see also Yearley et al., 2003) and the context of the modelling on the basis of their substantive knowledge of issues modelled (see Landström et al., 2011; Yearley, 1999, 2000; Yearley et al., 2001). What the empirical evidence suggests is that the rationale of the NSRAC fishing industry for taking part in the impact assessment exercise was that of a (co-)manager and knowledge-user – thus modelling as a ‘empirical friend’ – rather than that of ‘knowledge-(co)producer’ (see Callon, 1999), that is, embracing the opportunity to contribute to a more robust modelling foundations<sup>269</sup>. This was very different to the usual disputes over

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<sup>266</sup> Yet, as Yearley et al., 2001 claim, uncertainty and decision stakes cannot be measured. Sometimes, as Yearley (2006) shows, it can be as simple as stakeholders ‘falling’ for their modelling, so to speak. I am not saying this was the case here, where the fishing industry showed a more sophisticated engagement, as follows.

<sup>267</sup> It is not the individual active fishermen who participate in the RACs but, rather, their representatives, who can be fishermen retired from the sea but also biologists, economists or sociologists. More often than not, these are spokespeople with PhDs or MBAs. As reflected by Griffin and I can corroborate on the basis of my fieldwork, “the level of technocratic complexity in RAC meetings and working documents is still high, as is the level of scientific competence demanded” (2009:568; see also Hegland and Wilson, 2009). Notably, during my fieldwork, fishermen on interview sometimes expressed wariness towards their representatives at the NSRAC, from whom they felt distant.

<sup>268</sup> Some of the NSRAC spokespersons had indeed a background and work experience in biological modelling, so they did have further competence to deconstruct the shaky IMARES model if desired, at least to a certain degree (for research on this kind of ‘insertions’ see Landström et al., 2011).

<sup>269</sup> See Hegland and Wilson (2009) for some echoes of this argument in the case of the Pelagic RAC. It follows that bridging two knowledge cultures to enlarge the evidence base for policy advice may not ultimately describe how the RACs work – at least well enough. The problems with the CFP have

knowledge claims within the stock assessment exercises and the ‘heated’ challenging of the authority behind EU decisions on quotas<sup>270</sup>. Here the absence of tension was

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often been characterised as a ‘crisis of knowledge’ motivated by the inevitably different scales at which the stakeholders – particularly the fishing industry – and the scientists make their knowledge claims (Daw and Gray, 2005; Linke et al., 2011; Linke and Jentoft, 2013; Pálsson, 1998; Wilson, 2003). Yet, the dichotomy between systematic knowledge produced by scientists and anecdotal knowledge produced by the stakeholders is something socially constructed; the outcome of political work and, as such, the gulf does not always show up – at least at the level of RACs (see Griffin, 2013). The fishing industry is well aware that sometimes a solid way of gaining influence in the EU fisheries governance is to use the language and scales of the scientists – consistent with this strategy, they hire former scientists! Let us take the example of the surveys at sea described in §4.5. Scientists described their situated surveys that they then used to produce advice for the whole ICES fishing area – fisheries scientists carefully locate their work within a grid of so-called ICES fishing areas. The fishing industry did very much the same based on their inside out knowledge of those fishing spots where they go to find the fish year in year out. There was no strong evidence of two knowledge cultures driven by two inherently different scales of observation in the interaction between the NSRAC industry representatives and the scientists collaborating with them. They all looked as if they were part of the same ‘extended peer community’ (Funtowicz and Ravetz, 1993). This is one thing. Yet the evidence in this thesis points further at how the fishing industry may use science as part of their political work to leverage direct influence in decision-making and not just in the scientific advisory arena. As Griffin also captures it:

In attendance at NSRAC meetings I observed that instead of using anecdotal evidence or ‘fishy stories’ to lever support for their cause, representatives from the fishing industry regularly and often successfully, drew on science as a strategy for getting influence. Indeed, in many NSRAC discussions, fishing industry representatives tended to couch their arguments in the conventional scientific vernacular habitually deployed within this very discourse in order to censure the Commission if it had not, in the industry’s view, taken ‘adequate notice of the science’ (2009:570, emphasis in the original; see also Griffin, 2013).

As the argument goes, the evidence shows that the NSRAC is not just a ‘post-normal science’ organisation offering external peer review in reaction to the scientists’ inputs but goes beyond this notion. Notably, Hegland and Wilson (2009) provide empirical observations in this direction for the case of the Pelagic RAC but still frame the RACs as an ‘external peer community’. This thesis adds light to the discussion. Already in its early days the NSRAC needed to provide sound advice to the European Commission on often highly technical management proposals and engage in a close relationship with science. And in this context the NSRAC industry representatives revealed apparently ‘ambivalent’ in their approach to science; using ‘sound science’ as an ‘empirical friend’ (Yearley, 1991, 1992) to perform sophisticated engagement with particular management options (see Carter, 2013a, 2013b; Hegland and Wilson, 2009) while remaining generally uneasy with the epistemic characterisations of science as a truth-making activity and continue to criticise in ‘external peer review’ style the science used in the stock assessments. It is easy to understand though that the ambivalence was strategic, in both scenarios the NSRAC fishing industry was conducting political work.

<sup>270</sup> The rather ‘cool’ situation was indeed somehow closer to some social scientists’ accounts of Norwegian fisheries management, where there is no monolithic and coercive governing structure and the (modern) distinction between the governing system and the system-to-be-managed is much more blurred as a result of the direct involvement of the fishermen associations in the shaping of the management instruments – under a general context of more delegation of management to the fishermen (Johnsen, 2013; Johnsen et al., 2009; see also Holm et al., 2009). Indeed, it is probably no coincidence that the very same Dutch fishing industry leading the NSRAC to the co-management role



the result of both the lack of epistemic scrutiny of the IMARES model and the fact that the NSRAC industry stakeholders found IMARES ‘useful for political work’, that is, to mutually adjust their division of labour with respect to the producers of advice and policy-makers.

All in all, the integrated assessment modelling worked in practice as a space where the epistemic and organisational-political roles emerged in context, following the joint assessment of the representativeness and usefulness of the modelling. This might not be at first glance insightful given that IAM is conceived to be precisely a useful heuristic for policy – accuracy is therefore not a priority (Edwards, 1996a). Yet, this is not how things work in practice; as we know from STS, ‘working’ is not an intrinsic property. In this sense, I have described what it meant to be useful on the ground for the IAM in the context of the building a new framing for EU fisheries management. My claim is that integrated assessment modelling was ultimately useful as an organisational practice and a space for political work.

### **8.2.2 The role of integrated impact assessments**

It is the turn to discuss how the IIA worked as a policy instrument. Yet, to make the argument, I first need to recall the theoretical yardsticks, drawing largely on the Policy Studies literature and the interactionist strand of the co-production framework.

The IIA instrument was proposed as the operational procedure for the practical implementation of the European Commission’s Better Regulation strategy. The idea at the Commission behind the introduction of IIAs was the production of more

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is also the one that was already similarly used to the partial delegation of management – through the idiosyncratic ‘biesheuvel groups’ – back in their own country. By and large, the lesson is that the politics of stakeholder involvement or public engagement is always context dependent (Yearley, 2006, 2011; Yearley et al., 2003).

systematic, transparent and cross-cutting scientific evidence to depoliticise and legitimise EU policies; enabling the pre-empting of the political debate via proceduralisation as opposed to a political agreement on substantive issues of policy (Radaelli and Meuwese, 2008). Therefore, the EU approach to better governance was conceived on the basis of a technical-rational model of decision-making (Hertin et al., 2009a; Radaelli, 2004).

While the field of Policy Studies in Europe has paid dedicated attention to the role of IIAs in EU policy, it is necessary to attend to a significant split between two schools of thought with regard to how IIAs actually work – or should work. Some scholars – the so-called positivists (see Hertin et al., 2009a) claim there is a gap between the techno-rationalist ideal behind IIAs and the actual practice in the EU (see Renda, 2006). The argument is that, while pursuing better evidence, the IIA instrument often ends up weakening the evidence base because it can be easily challenged by unconstructive pressure groups wanting to safeguard pre-existing interests affected by the policy proposal(s) at stake. In other words, IIAs hit back in the sense that, “greater openness also allows greater scrutiny of factual issues in the assessments leading to the paradoxical situation where openness produces more criticism” (Bäcklund, 2009:1082; see also Wilkinson et al., 2005). As follows, the expectations of neutrality and objectivity, which in theory make IIAs amenable for improving EU regulation, do not hold in practice and IIAs end up undermining the techno-rationalist approach.

Yet, other scholars – the so-called post-positivists (see Hertin et al., 2009a) – argue that the inherent shortcomings are not the cause of the mismatch between the IIA’s theory and actual implementation. It is the narrow rationalist theory underpinning IIAs that is inadequate in the first place (Hertin et al., 2009a; Hertin et al., 2009b; Jacob et al., 2008; Owens et al., 2004; Radaelli, 2004, 2005). While, as Radaelli and Meuwese (2008; see also Radaelli and De Francesco, 2007) claim, IIAs can become tools for political control of the bureaucracy or, alternatively, tick-the-box exercises

with no influence on policy choices, Radaelli and Meuwese point out that IIAs tend to the greys between these two ends of the spectrum.

Indeed, Hertin et al. (2009a) advocate keeping both the technical-rational and the post-positivist visions as partially useful accounts of the role of IIAs. Along the same lines, Owens et al. (2004; see also Farrell et al., 2001; Hertin et al., 2009b; Jacob et al., 2008) stress that the by-product of using IIAs is often to open up politics for deliberation and learning across the different constituencies of the policy process. This is still consistent with a vision of IIAs as formal procedures, but there is also room for acknowledging how they facilitate conversation between political actors, thus enabling stakeholders to have a say in the policy process (Meuwese, 2008). Taking the argument further, what IIAs are ultimately able to provide is an answer to the question of who has authority in the regulatory process (Radaelli and Meuwese, 2008).

This thesis shows that the idiom of co-production can bring analytical depth to Radaelli and Meuwese's understanding of the IIA as a process confronted with the distribution of authority. As I have demonstrated, set up in principle as an objective knowledge practice free of political considerations, the IIA delivers in practice mutual adjustment between actors, which translates into a tension between depoliticisation and politicisation. How much tension is a contextual issue and not something intrinsic to the IIA instrument.

Recalling the main case study, the reform of the CFP in 2002, which brought in the NSRAC, and the Better Regulation strategy, which did so with the IIA instrument, triggered some new dynamics that became apparent for the first time in the case of the long-term management plan for the North Sea mixed flatfish fishery. The proposal for this long-term management plan arrived at a time when the scope and resources of the NSRAC as a provider of sound advice had yet to be defined – it was

too early days for the NSRAC. The European Commission expected the NSRAC to be scientific in its policy briefs and recommendations, which had to be the outcome of consensus. The NSRAC members embraced the importance of providing sound and consensual advice but, as a RAC, they had very limited formal access to a pool of scientific resources. Sometimes the NSRAC members would approach *ad hoc* their national governments for help with a particular issue, as happened with the Dutch ministry for fisheries affairs and the long-term management plan for the mixed flatfish fishery in the North Sea. Yet, formally, the NSRAC was meant to ask DG-FISH if they wished to get any scientific support. DG-FISH would then normally approach its own scientific pool of resources, that is, STECF, and request a working group to help the NSRAC.

The arrival of the IIA to DG-FISH – when the long-term management plan proposal for North Sea flatfish fishery was on the table – offered something of a rare opportunity. In principle, the IIA was an instrument in the hands of the DG-FISH to prove the soundness of its own regulatory proposals. In this respect, all that the NSRAC did – pairing with the Dutch government – in the first place was to ask DG-FISH to put the new instrument to use. However, it turned out that the NSRAC managed in the end to ‘domesticate’ (Lie and Sørensen, 1996) the new IIA instrument and produce their own proof for their preferred policy alternative, leveraging their authority in the policy process<sup>271</sup>. It was more than simply an exercise of political control of the bureaucracy<sup>272</sup>. The NSRAC fishing industry representatives – including a former scientist – could make a point about other policy choices. By and large, the IIA exercise became somehow an opportunity for a

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<sup>271</sup> Notably, this is in line with recent accounts of how the new policy instruments introduced with the reform of the CFP in 2002 gave the industry stakeholders direct influence in decision-making (Griffin, 2013).

<sup>272</sup> Nonetheless, there was an element of this kind of control when it comes to timescales. By requesting an IIA the NSRAC fishing industry could slow down the policy process in order to leverage more influence (see Griffin, 2013).

reversal of the burden of proof<sup>273</sup>. Notably, on this evidence, there was no room for the critique over the paradoxical character of IIAs in the sense portrayed by Bäcklund (2009; see also Wilkinson et al., 2005). Rather than the IIA resulting only in unconstructive criticism of existing options, the exercise brought in evidence of other options. Yet this evidence did not confirm one policy option as objectively the best – and thus help to depoliticise the management decision. Instead, there were two alternatives on the table and politicisation over which one was better. All in all, how the IIA exercise worked was marked by the tension between politicisation and depoliticisation that emerged in this particular context.

### **8.2.3 The role of harvest control rules**

Harvest control rules are the third element of the troika of instruments under the spotlight in this thesis. I will also draw some conclusions over how they worked in practice, this time using not only the evidence from the case of the North Sea flatfish fishery but also from the case of the managing of the North East Arctic cod fishery.

The rationale for the regulatory reform of the CFP in 2002 was that policy decisions could be made on the basis of widely agreed long-term management plans grounded on sound HCRs. This should provide a thorough change to the politically informed decisions under the original CFP, which tended to factor in the short-term interests of the fishing industry (Holden, 1994; Lequesne, 2004). By and large, the idea was to depoliticise the decision-making process. In a way, the more EU fisheries resembled a gigantic fish farm with long-term production plans and clear divisions of labour to facilitate decisions, the closer to the ideal. Stakeholders had to be consulted on the

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<sup>273</sup> Hegland and Wilson (2009) make a similar observation for the case of the establishment of a long-term management plan for the western horse mackerel in the Pelagic RAC. It is worth noticing that the reversal of the burden of proof is meant to be an element of the new reform of the CFP, passed at the time of writing these lines and ready to enter in operation on 1 January 2014 (European Commission, 2013b). See Linke and Jentoft (2013) for a discussion on the future implications and challenges of this approach in EU fisheries governance and the expected key role to be played by the RACs.

shape of the long-term management plans and, once approved, the TAC Machine with its annual stock assessments should be able to deliver a 'better job' under much more predictable and stable conditions granted by the application of the same HCR year after year. According to the HCR, TACs were meant to decrease over the years to bring the fish stocks up to targeted levels – high yields – but the inter-annual variability should remain within certain thresholds agreed with the RAC to give the fishing industry some stability. Never mind that the TACs had not been an effective instrument to help the conservation of the fish stocks and had brought conflict with the fishing industry, they were entrenched in the EU fisheries management system (Holm and Nielsen, 2004). The TAC Machine was in practice the foundation on which the whole principle of relative stability was orchestrated – and the CFP could not be reformed without preserving this principle. The machinery was entrenched simply because it served the distribution of the quota shares in tons of fish for each fishery under the CFP. Nonetheless, in order to ensure the conservation of the fish stocks and indeed a steady recovery towards high yields, the new long-term management plans in the EU ruled that fishing effort limitations – i.e. days at sea – should complement the TACs.

This is how the rationale of the 2002 reform looked in a nutshell. However, on the evidence presented, the HCR for the North Sea flatfish fishery did not live up to the expectations of depoliticising the decision-making process at the EU. The introduction of HCR did not largely change the degree of politicisation in the decision-making process. The HCR was applied, but in response to the interests of the fishing industry, and not without political bargaining with Norway. A similar pattern emerged in the case of the North East Arctic cod fishery and the Joint Norwegian-Russian Fisheries Commission. In a similar way to what I argued for the IIAs, applying the HCRs was not simply calculating the TACs for the following year but also reassessing who-really-had-authority-when – a case of interactionist co-production.

While it was perhaps too early days to expect any significant change in the depoliticisation of EU fisheries policy, the implementation of HCR pointed to a pattern in fisheries policy, at least in the EU; exceptions to the governing rules tend to become the norm<sup>274</sup>. Commenting on the struggles with implementing regulatory measures, Griffin (2010) claims that EU fisheries management lives indeed on perennial ‘states of exception’ brought about by competing actors – such as the Commission, the Council of Ministers, the Member States, the fishing industry, and the environmental NGOs – all striving to achieve authority. As the argument goes, there are always good (authoritative) reasons why agreed procedures in Brussels cannot be followed in practice on this or that occasion.

By and large, while HCRs had been conceived as a response to the excessively politicised decisions at the Council of Ministers of the EU or at the Joint Norwegian-Russian Fisheries Commission, what the implementation of HCRs offered at the time of research was some kind of a *déjà vu*. The decision-making process was still heavily politicised; the main difference was that the political discussion had moved to whether to apply the rule or not, but the tension between depoliticisation and politicisation remained higher than intended with the establishment of the HCRs.

All in all, I have illuminated the spaces for political work that the new technologies for the depoliticisation of decisions in EU fisheries management ‘opened up’ (after Stirling, 2008). By and large, while these new instruments of fisheries management were designed to be ‘anti-political’ (Barry, 2002) in order to mechanise policy decisions, they did not deliver in such a clinical way once in the context of application. I have detailed how those new spaces were ‘domesticated’ (Lie and Sørensen, 1996) by scientists, stakeholders and policy-makers. What policy

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<sup>274</sup> See Hegland and Wilson (2009) for an account of an exception to the rule in the case of the implementation of the HCR for the western horse mackerel fishery in 2008, that is, straight after the long-term management plan for this fishery was approved.

innovation for the building of a new version of the sociotechnical framing brought in to EU fisheries policy was the revamping – to different degrees of tension – of the relationships between science and policy, science and stakeholders and in-between scientific advisors from different disciplines, but no substantial reform as yet of the muddled way decisions were reached in Brussels. All in all, the central message is that the new technologies of governing introduced to depoliticise were in effect political technologies (Barry, 2002; Lascoumes and Le Galès, 2007).

### **8.3 How are EU fisheries made governable? Landing a weighty by-catch**

The study of how the troika of instruments for the reframing of EU fisheries worked links to the discussion of how EU fisheries are made governable. Admittedly, this is a rather general question to arrive at, which stems from taking the analysis to the level of the policy process as opposed to looking at the model for policy simply as an artefact. Given that this is not part of my original or revised set of research questions, I shall refer to it as substantial ‘by-catch’ of my research.

Social scientists working on fisheries often reflect on fisheries governance, not only by describing how it works but also by suggesting how it should work to remedy its most common shortcomings (Degnbol et al., 2006; Nielsen and Holm, 2007; Schwach et al., 2007; Symes, 2001, 2006). Yet, at least two distinctive schools of thought co-exist.

On the one hand, there is a strand of social scientists who, influenced by governance theory, believe that the problem lies in the insufficient mutual adjustment between the management system – instruments and institutions – and the external issues to be managed – observed indirectly by a system of representation (Berkes, 2010; Jentoft, 2007; Kooiman et al., 2005). It is through the adjustment of the management system to the system-to-be-managed that governability is enabled. In this sense, the



governing system and the system-to-be-governed have to be isomorphic, at least as much as possible – for instance, it is incompatible to manage a mixed fishery with single stock-oriented policy instruments. For these scholars the problem is, precisely, how unsuitable the instruments of fisheries management tend to be for the complex environment that they are meant to deal with. On the other hand, there is another strand of social scientists working with fisheries that claim that governability is enabled by a constitutive co-production of the governing system and the-system-to-be-governed. They claim that the governing of a fishery is not determined by however technically unsophisticated the policy instruments may be in response to the perceived complexities of the particular fishery; the instruments are not applied to the world out there anyway. As follows, the key to understand how fisheries are made governable is to focus on how the instruments are used to produce governable objects and actions (Holm, 2007; Johnsen, 2013; Johnsen et al., 2009). That implies concentrating on the framing (Callon, 1998; see also Holm and Nielsen, 2007) that tries to stabilise the objects and actions of management.

In EU fisheries management, the TAC Machine (Holm and Nielsen, 2004) was the sociotechnical framing *par excellence* until 2002, at least as conceived originally, that is, delivering manageable single fish stocks divisible in tons (quotas). Two major negative externalities affected the whole mechanistic enterprise, however<sup>275</sup>. These were the failure of the conservation of the fish stocks and the alienation of the fishermen. The first problem stemmed, at least in the view of the guardians of the Common Fisheries Policy at DG-FISH, from the fact that decisions were based on bargaining games between EU member states on the back of the individual fish stocks to safeguard the interests of their national fleets. One of the main reasons the CFP was reformed in 2002 was to solve this problem. In short, the reframing efforts consisted of bringing in more science through ‘anti-political’ (Barry, 2002) instruments to depoliticise fisheries decisions. Depoliticisation was, therefore,

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<sup>275</sup> Notably, without derailing the TAC Machine as such, which remained politically useful to operationalise the principle of relative stability (Holm and Nielsen, 2004).

nothing more and nothing less than to protect fisheries policy rational decisions from the negotiation skills of the national political authorities that sit on the Council of Ministers in the same way that the fish needed to be protected from the effective catching capacity of the fishermen. Meanwhile, moving to the second problem, the fishermen, who can sense earlier than anyone else the fluctuations in the amount of fish and the migration patterns, were excluded of the management system during the reign of the original version of the TAC Machine, which was conceived to be fuelled only by scientists. This changed with the reform of the CFP in 2002 as well; fishermen were now expected to contribute with their experience-based advice to the rational decision-making enterprise. Moreover, they could not only provide first-hand information about the fish stocks but also about their fishing practices out at sea. Therefore, under the reframing efforts, the fishermen were invited to the management system – whereas before they were just part of the system-to-be-managed (Johnsen et al., 2009). However, it is important to bear in mind that, in the EU context, this invitation was limited to improving the knowledge base for rational decision-making.

By and large, the reform of the CFP in 2002 ambitioned a new constitutive co-production of new natural and social orders to allow rational and depoliticised decisions – the original sociotechnical framing, the TAC Machine was there to stay by virtue of some political usefulness but it was clearly not good enough. Put vividly, the reframing efforts of EU fisheries management aimed at (gigantic) multi-species ‘fish-farms’ directly in the open sea, offering high yields by means of harvest control rules governing both the fish and the fishermen. By and large, focusing on sociotechnical framings and their particular co-productions of the management-system and the system-to-be-managed seems a fruitful way to explain how fisheries can be governed.

Meanwhile, how a framing ultimately works depends on how useful or problematic it is publicly evaluated rather than on its technical sophistication to match or be

compatible with the complexity perceived in the real world. Context is key since generating framings does not ever begin with a clean slate. Sometimes the framing works more or less straight and constitutively co-produces manageable fish and fishermen like in the case from the ‘cool’ waters of the Norwegian Sea (see Holm, 2007; Johnsen, 2013), sometimes less so, like in the more ‘heated’ case of the EU. In the often-contested EU fisheries policy arena, the tools for depoliticisation tend to ‘frame’ poorly and ‘overflow’ or politicise considerably (after Callon, 1998, 2004). In other words, constitutive co-production often delivers rather shaky ‘scaffoldings’ (Pollock and Williams, 2007) for management decisions, bringing along negative externalities with the characterisation of the manageable fish and fishermen, which then have to be tamed.

In the empirical chapters I offered evidence of how the process to transform the North Sea flatfish fishery into a mechanistic ‘fish farm’ for sole and plaice governed by a harvest control rule worked. This reframing process did not produce governable fishermen willing to accept the governing instruments and organise themselves around them in mechanistic fashion (Burns and Stalker, 1994[1961]). At one point, actors reached stagnation; yet two years had passed by, and this started to create serious inconveniences to the fishermen as well as to their representatives in the NSRAC. Given the lengthy policy process and the economic uncertainty that this represented for the fleets already hit by high fuel prices, and given as well that the political elections in the Netherlands that could bring in a fisheries minister of a very different colour (green), the fishing industry urged for an agreement and the closure of the policy process. Meanwhile, DG-FISH was also keen on putting the IIA to the side and find a much-needed agreement for the health of fish stocks. After two years of discussions and an IIA exercise that had raised serious doubts within DG-FISH, officials were keen to negotiate directly with the fishing industry again without the proxy of the IIA – which they could do away with legally by declaring the emergency of the situation for the flatfish fish stocks. It was an acceptable degree of politicisation for all parties and made possible the decision on the long-term management plan proposal for the North Sea flatfish fishery.

On this evidence, I argue that it is this tension between depoliticisation and politicisation under the framing efforts that drives the governing of a fishery. Politicisation is the result of the mutual adjustments between the framing and the policy process – upon which the framing acts and that has to bear the externalities. When this politicisation is considered by the actors to be acceptable, fisheries can be governed by means of the sound policy instruments that support the framing. It is when the politicisation builds up, when the design and implementation of the instruments open up fundamental questions about the distribution of authority, that the limits to the governability of fisheries are exposed<sup>276</sup>. In EU fisheries – but also in other international fishing grounds like the Barents Sea – this tension between depoliticisation and politicisation tips often towards the latter and leads to ‘states of exception’ (Griffin, 2010) as the normal way to reach decisions.

All in all, the interest of EU fisheries as a topic is precisely that no single actor is able to frame and stabilise the policy process for a long time because another actor(s) will be bearing some negative externalities. The tension between depoliticisation and politicisation is the norm no matter the degree of sophistication of the policy instruments – including the modelling – introduced in response to holistic views of the fisheries. This is why the Common Fisheries Policy, the framework to move from muddily governed fisheries into more mechanistic relationships, is always in the process of being tuned up. And somehow, a key factor to keep the tension on workable levels and allow governability is to have the fishing industry in the co-management role (see Griffin, 2013; Johnsen, 2013; see also Carter, 2013b) – that is, in contrast to the modern and mechanistic distinction between those who manage and those who are managed, which frames the fishing industry in the role of providers of additional relevant data and advice at best.

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<sup>276</sup> In the language of Callon (1998; see also Callon et al., 2009) this would mean a situation where overflows, that which shakes the bounds of existing frameworks, dominate the policy process.

Before moving on to the last section of the chapter there is a last and much more exploratory reflection to make. This thesis hints at a pragmatic version of the EU<sup>277</sup> that is fundamentally practised as a collection of spaces where actors come together and expose the tension between depoliticisation and politicisation while they participate in the construction and resolution of European problems (see Rowell and Mangenot, 2011). While generalising from one setting to the whole of the EU could be always regarded as problematic, what the main case study illuminates is the hypothesis that other EU sectors may be governable in the same way a fishery is; that is, by means of evidence-based policy instruments that offer a polity where the distribution of authority through political work enacts the temporary and local assembly of the EU within the constant wrestling characteristic of EU regulated sectors (see Carter, 2013b; Carter and Smith, 2008). As argued by Waterton and Wynne (2004), the framework of co-production can help to account for the crafting of these institutional local forms within broader conflicting forces and adjustments, something that these scholars do effectively for the European Environment Agency. As follows, very much as the Common Fisheries Policy or the European Environment Agency, the EU may be regarded as an institution-always-in-the-making. In this respect, this thesis speaks to a still incipient cross-disciplinary analysis of the EU in which knowledge-practices and technologies of governing receive enough attention as contributors to the (re-)ordering of Europe as a political entity (Barry, 2001; Callon, 2004; Carter and Lawn, forthcoming; Halpern, 2010; Jasanoff, 2005; MacKenzie, 2009; Rowell and Mangenot, 2011; Smith, 2013; Sundqvist and Letell, 2005; Waterton and Wynne, 2004). All in all, the idea of the co-production of knowledge-expertise and the European Union deserves further research<sup>278</sup>.

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<sup>277</sup> As opposed to a centralist version of policy order driven by the idea of a harmonised European Union through formal legalistic assumptions of authority (see Waterton and Wynne, 2004).

<sup>278</sup> It should not come as a surprise that the 'by-catch' of this research journey points to further investigation since, following with the metaphor, by-catch can lead to spotting new opportunities away from already intensively 'trawled' topics. One possible programmatic approach to the unpacking of the co-production of knowledge-expertise and the EU that this thesis hints at is to look into how modelling, as part of particular sociotechnical framings, offers spaces where the EU is

## **8.4 Coda**

In the same way that the single fish stock is no longer the object of exclusive attention in EU fisheries policy at the expense of the mixed fishery, this thesis has developed by moving away from its originally intended single object of inquiry. Looking with the lenses of co-production at the level of the policy process, I have produced an analysis that avoids presuming that modelling is central for policy by means of foregrounding the sociotechnical framing. Notably, this is the opposite stand in general to the analysis of ‘computer models as artefacts’ characteristic of those social constructivists in STS commenting on the representational validity of modelling for policy-making. It may be argued that they have collectively contributed to enacting the models’ apparent pivotal stature in the policy process by drawing attention to their social construction with a vast stream of work (Edwards, 1999, 2010; Evans, 2002; Lahsen, 2005; Petersen, 2006, 2008; O’Reilli et al., 2012; Shackley, 1997, 2001; Shackley et al., 1998, 1999; Shackley and Wynne, 1995b, 1996; Van der Sluijs et al., 1998). Unfolding the social construction of the knowledge generated by means of modelling for policy has been their way to suggest that things could have been different because they could have always been modelled under different conditions – modellers always take situated epistemic assumptions and styles of modelling (see Shackley, 2001). In this respect, a related stream of work has provided strong arguments for producing ‘good’ modelling for policy, and thus more robust policies, by opening up the building of the models to non-modellers or non-specialists at early stages to review their epistemic (inner) quality and increase their legitimacy (Dahinden et al., 2000, 2003; Darier et al., 1999; Siebenhuner and Barth, 2005; Van der Sluijs, 2002b; Van der Sluijs et al., 2005; see also Yearley et al., 2003). By and large, ‘participatory modelling’ offers a gold mine for case studies making this general and important point about any technology design. However, in the policy context, ‘things could have always been (modelled)

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locally practised. Such a research programme may thus direct attention to the (mediated and contextual) role of modelling in the ordering of different versions of the EU across different sectors and geographies.

differently' is an argument that, at least tacitly, assumes the modelling to be central to the policy process in order to shine, otherwise it shows the imprint of what Stewart and Williams (2005; see also Williams, 2006) call a narrative bias.

Indeed, STS commentators working on modelling for policy in the constructivist tradition often assume too much of a pivotal position of the modelling in the policy process due to the compelling persuasiveness of real-looking representations. Alternatively, sometimes the centrality towards policy is not necessarily on the basis of representations, for instance when models are presented as boundary objects acting as organisational 'focal points' for coordination between science and policy (Edwards, 2001, 1996a), yet the narrative bias remains<sup>279</sup>. Speaking to these narratives, I have stressed that presuming centrality may well result in missing 'the forest for the trees'<sup>280</sup>, a risk that vanishes when the sociotechnical infrastructure that enables policy decisions is foregrounded.

Holm and Nielsen (2004; see also Nielsen, 2008) already set the record straight with their exemplary account of the TAC Machine, that is, the (co-productionist) establishment of a sociotechnical framing to enable quotas in tons of fish with the help of the Virtual Population Analysis. Based on information about the cohorts, this was the preferred approach to calculate fish stock population numbers as opposed to producing estimates on the basis of how much effort it takes vessels to catch the fish. The key point is that the TAC-VPA coupling became the favoured approach in the ICES world back in the mid 1970s not because its particular attributes were interpreted to represent better the amount of fish in the sea, but because of granting in principle a neat adjustment or division of labour between science advising on TACs and policy-makers deciding on quotas. The TAC Machine became adopted as

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<sup>279</sup> Though exceptions exist within the STS social constructivist strand that go beyond this bias (for instance, when it comes to the exemplary case of climate change policy-making, see Yearley, 2009; Wynne, 2010, Demeritt, 2001; see also Shackley and Darier, 1998).

<sup>280</sup> Or as, Holms also puts metaphorically, modeling is "tip of the iceberg" (2007:238).

the framing for annual quota decisions when the CFP was established in 1983. By and large, the VPA-TAC made it possible to fulfil the visions of the masterminds of a CFP for the distribution of fishing rights following the golden principle of relative stability – conservation was only a façade (Holden, 1994; Nielsen and Holm, 2007) – and the CFP proved the TAC Machine a success in that particular respect. And the fact is that, despite failing conservation or being heavily criticised because of the alienation of the fishing industry, the VPA-TAC framing became locked in as the convenient way to set the boundaries for a stable annual distribution of quotas across the EU Membership. All in all, the fact that the VPA modelling became entrenched was a contextual outcome; the consequence of becoming a ‘useful’ part of the dominant sociotechnical framing for EU fisheries decisions despite a number of well-known limitations – and insight that an analysis centred on the VPA modelling as an artefact would most certainly miss.

All in all, modelling for policy works as a space within the wider sociotechnical framing efforts that may enable decision-making. Representational validity and organisational usefulness emerge within that space, co-produced in the context of building the serviceable sociotechnical framing that makes decisions possible. How modelling for policy-making matters is, therefore, a mediated and contextual outcome that stems from how convenient it is to the sociotechnical framing that it is part of and shapes. Modelling does not speak directly to policy. As a distinctive contribution, this thesis thus questions the presumption in many social constructivist accounts that modelling alone becomes central to the policy process and its outcomes. The significance of modelling for policy-making should be understood in terms of its contribution to processes of sociotechnical framing. Narratives that foreground the former and background the latter show an analytical bias that needs turning around.





## 9 List of references

- Abbott, A., 1992. What Do Cases Do? Some Notes on Activity in Sociological Analysis. In C. C. Ragin & H. S. Becker, eds. *What is a Case? Exploring the Foundations of Social Inquiry*. Cambridge: Cambridge University Press, pp. 53–81.
- Anand, R.P., 1982. *Origin and Development of the Law of the Sea: History of International Law Revisited*, Dordrecht, the Netherlands: Kluwer.
- Anderson, E.D., 1998. The History of Fisheries Management and Scientific Advice – The ICNAF / NAFO History from the End of World War II to the Present. *Journal of Northwest Atlantic Fisheries Science*, 23, pp.75–94.
- Aranda, M. & Motos, L., 2006. Strategy Evaluation (MSE) and Management Procedure (MP) Implementations in Practice: A Review of Constrains, Roles and Solutions. In L. Motos & D. C. Wilson, eds. *The Knowledge Base for Fisheries Management*. Amsterdam: Elsevier, p. 454.
- Asdal, K., 2008. On Politics and the Little Tools of Democracy: A Down-to-Earth Approach. *Distinktion: Scandinavian Journal of Social Theory* (16), pp.11–26.
- Bäcklund, A.-K., 2009. Impact Assessment in the European Commission – A System with Multiple Objectives. *Environmental Science & Policy*, 12(8), pp.1077–1087.
- Barnes, B., Bloor, D. & Henry, J., 1996. *Scientific Knowledge: A Sociological Analysis*, Chicago, Illinois: University of Chicago Press.
- Barry, A., 2001. *Political Machines: Governing a Technological Society*, London: The Athlone Press.
- Barry, A., 2002. The Anti-Political Economy. *Economy and Society*, 31(2), pp.268–284.
- Barry, A., Bell, V. & Rose, N., 1995. Alternative Political Imaginations. *Economy and Society*, 24(4), pp.485–488.
- Barry, A., Born, G. & Wieszkalnys, G., 2008. Logics of Interdisciplinarity. *Economy and Society*, 37(1), pp.20–49.
- Bavington, L.D.Y., 2009. Managing to Endanger: Creating Manageable Cod Fisheries in Newfoundland & Labrador, Canada. *Maritime Studies*, 7(2), pp.99–121.
- Beck, U., 1992. *Risk Society: Towards a New Modernity*, London: Sage.

- Beck, U., 1997. Subpolitics. *Organization & Environment*, 10(1), pp.52–65.
- Beder, S., 1998. *The New Engineer: Management and Professional Responsibility in a Changing World*, South Yarra: Macmillan.
- Bell, J., 2010[1987]. *Doing Your Research Project* 5th ed., Maidenhead: Open University Press.
- Berkes, F., 2010. Shifting Perspectives on Resource Management: Resilience and the Reconceptualization of “Natural Resources” and “Management”. *Maritime Studies*, 9(1), pp.13–40.
- Bertilsson, T.M., 2004. The Elementary Forms of Pragmatism: On Different Types of Abduction. *European Journal of Social Theory*, 7(3), pp.371–389.
- Beverton, R.J.H. & Holt, S., 1993[1957]. *On the Dynamics of Exploited Fish Populations* reprint, London: Chapman and Hall.
- Bickerstaff, K. & Simmons, P., 2004. The Right Tool for the Job? Modeling, Spatial Relationships, and Styles of Scientific Practice in the UK Foot and Mouth Crisis. *Environment and Planning D: Society and Space*, 22(3), pp.393–412.
- Bijker, W.E., 1995. *Of Bicycles, Bakelites, and Bulbs: Toward a Theory of Sociotechnical Change*, Cambridge, Massachusetts: MIT Press.
- Bijker, W.E., 2006. Why and How Technology Matters. In R. E. Goodin & C. Tilly, eds. *Oxford Handbook of Contextual Political Analysis*. Oxford: Oxford University Press, pp. 681–706.
- Bijker, W.E., Hughes, T. & Pinch, T. eds., 1987. *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*, Cambridge, Massachusetts: The MIT Press.
- Bijker, W.E. & Law, J. eds., 1992. *Shaping Technology, Building Society: Studies in Sociotechnical Change*, Cambridge, Massachusetts: MIT press.
- Blaikie, N., 2007. *Approaches to Social Enquiry: Advancing Knowledge* 2nd ed., Cambridge: Polity.
- Blok, A., 2010. *Divided Socio-Natures: Essays on the Co-Construction of Science, Society, and the Global Environment*. Thesis (PhD). University of Copenhagen. Available from: [www.dasts.dk/wp-content/uploads/Anders-Blok-2010.pdf](http://www.dasts.dk/wp-content/uploads/Anders-Blok-2010.pdf) [Accessed 29 October 2013].
- Bloor, D., 1991[1976]. *Knowledge and Social Imagery* 2nd ed., Chicago: University of Chicago Press.

- Bloor, D., 1999. Anti-Latour. *Studies in History and Philosophy of Science*, 30(1), pp.81–112.
- Bolster, W.J., 2006. Opportunities in Marine Environmental History. *Environmental History*, 11(3), pp.567–597.
- Borges, J.L., 1998[1942]. Funes the Memorious. In *Collected Fictions*, translated by Hurley, A. London: Penguin Books, pp. 131–137.
- Boswell, C., 2009. *The Political Uses of Expert Knowledge*, Cambridge: Cambridge University Press.
- Bowker, G. & Star, S.L., 1999. *Sorting Things Out: Classification and Its Consequences*, Cambridge, Massachusetts: MIT Press.
- Briquet, J.-L., 1994. Communiquer en Actes. Prescriptions de Rôle et Exercice Quotidien du Métier Politique. *Politix*, 7(28), pp.16–26.
- Brunner, R.D., 1996. Policy and Global Change Research. *Climatic Change*, 32(2), pp.121–147.
- Burgess, R.G., 1984. *In the Field: An Introduction to Field Research*, New York: Routledge.
- Burns, T. & Stalker, G.M., 1994[1961]. *The Management of Innovation* rev. ed., Oxford: Oxford University Press.
- Byrne, D., 2005. Complexity, Configurations and Cases. *Theory, Culture & Society*, 22(5), pp.95–111.
- Caddy, J.F. & Cochrane, K.L., 2001. A Review of Fisheries Management Past and Present and Some Future Perspectives for the Third Millennium. *Ocean & Coastal Management*, 44(9), pp.653–682.
- Callon, M., 1986. Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Brieuc Bay. In J. Law, ed. *Power, Action and Belief: A New Sociology of Knowledge?* London: Routledge, pp. 196–223.
- Callon, M., 1987. Society in the Making: The Study of Technology as a Tool for Sociological Analysis. In W. Bijker, T. Hughes, & T. Pinch, eds. *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*. Cambridge, Massachusetts: MIT Press, pp. 83–103.
- Callon, M., 1998. An Essay on Framing and Overflowing: Economic Externalities Revisited by Sociology. In M. Callon, ed. *The Laws of the Markets*. London: Blackwell, pp. 244–269.

- Callon, M., 1999. The Role of Lay People in the Production and Dissemination of Scientific Knowledge. *Science Technology & Society*, 4(1), pp.81–94.
- Callon, M., 2004. Europe Wrestling with Technology. *Economy & Society*, 33(1), pp.121–134.
- Callon, M., 2005. Why Virtualism Paves the Way to Political Impotence: A Reply to Daniel Miller’s Critique of the Laws of the Markets. *Economic Sociology European Electronic Newsletter*, 6(2), pp.3–20.
- Callon, M., 2006. What Does it Mean to Say that Economics is Performative? In D. A. MacKenzie, F. Muniesa, & L. Siu, eds. *Do Economists Make Markets? On the Performativity of Economics*. Princeton, New Jersey: Princeton University Press.
- Callon, M. & Latour, B., 1992. Don’t Throw the Baby Out With the Bath School! A Reply to Collins and Yearley. In A. Pickering, ed. *Science as Practice and Culture*. Chicago, Illinois: University of Chicago Press, pp. 343–368.
- Callon, M. & Law, J., 1995. Agency and the Hybrid “Collectif”. *The South Atlantic Quarterly*, 94(2), pp.481–507.
- Callon, M., Lascoumes, P. & Barthe, Y., 2009. *Acting in an Uncertain World: An Essay on Technical Democracy*, Cambridge, Massachusetts: MIT Press.
- Campbell, B.L., 1985. Uncertainty as Symbolic Action in Disputes Among Experts. *Social Studies of Science*, 15(3), pp.429–453.
- Cardinale, M. & Svedäng, H., 2008. Mismanagement of Fisheries: Policy or Science? *Fisheries Research*, 93(1–2), pp.244–247.
- Carter, C., 2013a. Constructing Sustainability in EU Fisheries: Re-Drawing the Boundary between Science and Politics? *Environmental Science & Policy*, 30, pp.26–35.
- Carter, C., 2013b. The Transformation of Scottish Fisheries: Sustainable Interdependence from “Net to Plate.” *Marine Policy* [online]. Available from: <http://dx.doi.org/10.1016/j.marpol.2013.08.014i>. [Accessed 29 October 2013].
- Carter, C. & Lawn, M. eds., forthcoming. *Governing Europe’s Spaces: European Union Re-imagined*, Manchester: Manchester University Press.
- Carter, C. & Smith, A., 2008. Revitalizing Public Policy Approaches to the EU: “Territorial Institutionalism”, Fisheries and Wine. *Journal of European Public Policy*, 15(2), pp.263–281.

- Castells, N. & Ravetz, J., 2001. Science and Policy in International Environmental Agreements: Lessons from the European Experience on Transboundary Air Pollution. *International Environmental Agreements: Politics, Law and Economics*, 1(4), pp.405–425.
- Clark, W.C. et al. eds., 2001. *Learning to Manage Global Environmental Risks Volume 1: A Comparative History of Social Responses to Climate Change, Ozone Depletion, and Acid Rain*, Cambridge, Massachusetts: MIT Press.
- Clarke, A. & Fujimura, J. eds., 1992. *The Right Tools for the Job*, Princeton, New Jersey: Princeton University Press.
- Clover, C., 2004. *The End of the Line: How Overfishing is Changing the World and What We Eat*, London: Ebury.
- Cobb, C.W. & Douglas, P.H., 1928. A Theory of Production. *The American Economic Review*, 18(1), pp.139–165.
- Cochrane, K.L. & Doulman, D.J., 2005. The Rising Tide of Fisheries Instruments and the Struggle to Keep Afloat. *Philos Trans R Soc Lond B Biol Sci.*, 360(1453), pp.77–94.
- Collingridge, D. & Reeve, C., 1986. *Science Speaks to Power: The Role of Experts in Policy Making*, London: Pinter.
- Collins, H.M., 1988. Public Experiments and Displays of Virtuosity: The Core-Set Revisited. *Social Studies of Science*, 18(4), pp.725–748.
- Collins, H.M., 2004. How Do You Know You've Alternated? *Social Studies of Science*, 34(1), pp.103–106.
- Collins, H.M. & Evans, R., 2002. The Third Wave of Science Studies: Studies of Expertise and Experience. *Social Studies of Science*, 32(2), pp.235–296.
- Collins, H.M. & Yearley, S., 1992. Epistemological Chicken. In A. Pickering, ed. *Science as Practice and Culture*. Chicago, Illinois: University of Chicago Press, pp. 301–326.
- Council of the European Union (2002). Council Regulation (EC) No 2371/2002 of 20 December 2002 on the Conservation and Sustainable Exploitation of Fisheries Resources under the Common Fisheries Policy. *Official Journal of the European Communities*, L 358/59. 13.12.2002. pp.59–80.

- Council of the European Union (2007). Council Regulation (EC) No 676/2007 of 11 June 2007 Establishing a Multiannual Plan for Fisheries Exploiting Stocks of Plaice and Sole in the North Sea. *Official Journal of the European Union*, L 157. 19.06.2007. pp.1–6.
- Cozzens, S.E. & Woodhouse, E.J., 1995. Science, Government, and the Politics of Knowledge. In *Handbook of Science and Technology Studies*. London: Sage, pp. 533–553.
- Craige, B.J., 1978. *Selected Poems of Antonio Machado*, Baton Rouge: Louisiana State University Press.
- Da Condeição-Heldt, E., 2004. *The Common Fisheries Policy in the European Union: A Study in Integrative and Distributive Bargaining*, London: Routledge.
- Dahinden, U. et al., 2003. Citizen Interaction with Computer Models. In B. Kasemir et al., eds. *Public Participation in Sustainability Science: A Handbook*. Cambridge: Cambridge University Press, pp. 105–125.
- Dahinden, U. et al., 2000. Exploring the Use of Computer Models in Participatory Integrated Assessment: Experiences and Recommendations for Further Steps. *Integrated Assessment*, 1(4), pp.253–266.
- Darier, E., Shackley, S. & Wynne, B., 1999. Towards a “Folk Integrated Assessment” of Climate Change? *International Journal of Environment and Pollution*, 11(3), pp.351–372.
- Davies, G., 2006. Mapping Deliberation: Calculation, Articulation and Intervention in the Politics of Organ Transplantation. *Economy and Society*, 35(2), pp.232–258.
- Daw, T. & Gray, T., 2005. Fisheries Science and Sustainability in International Policy: A Study of Failure in the European Union’s Common Fisheries Policy. *Marine Policy*, 29(3), pp.189–197.
- Degnbol, D. & Wilson, D.C., 2008. Spatial Planning on the North Sea: A Case of Cross-scale Linkages. *Marine Policy*, 32(2), pp.189–200.
- Degnbol, P., 1998. Fisheries Research in Development. In: The Research Council of Norway, *Conference on Fisheries Research in Developing Countries*. Oslo, 21–22 October 1998.
- Degnbol, P., 2004. Fisheries Science in a Development Context. In B. Hersoug, S. Jentoft, & P. Degnbol, eds. *Fisheries Development: The Institutional Challenge*. Delft, the Netherlands: Eburon, pp. 131–155.

- Degnbol, P. et al., 2006. Painting the Floor with a Hammer: Technical Fixes in Fisheries Management. *Marine Policy*, 30(5), pp.534–543.
- Delaney, A.E. & Hastie, J.E., 2007. Lost in Translation: Differences in Role Identities between Fisheries Scientists and Managers. *Ocean & Coastal Management*, 50(8), pp.661–682.
- Demeritt, D., 2001. The Construction of Global Warming and the Politics of Science. *Annals of the Association of American Geographers*, 91(2), pp.307–337.
- Dessai, S. & Hulme, M., 2004. Does Climate Adaptation Policy Need Probabilities? *Climate Policy*, 4(2), pp.107–128.
- DG-FISH, 2007a. *Letter to the NSRAC – ICES Advice on North Sea Plaice and Sole*. Directorate-General for Fisheries and Maritime Affairs, Brussels, Belgium. 17.12.2007.
- DG-FISH, 2007b. *Scientific Advice and Stakeholder Group – Information Note*. Directorate-General for Fisheries and Maritime Affairs, Brussels, Belgium. 08.03.2007.
- DG-MARE, 2012. *Roadmap: Revision of the Multiannual Plan for Fisheries Exploiting Stocks of Plaice and Sole in the North Sea*. Directorate-General for Maritime Affairs and Fisheries, Brussels, Belgium. November 2012.
- Directorate of Fisheries, 2007. *Status Report for 2006 – Russian Fishing of Cod and Haddock*. Directorate of Fisheries, Bergen, Norway. Available from: [www.regjeringen.no/Upload/FKD/Vedlegg/Diverse/2007/Status%20report%20-%20Russian%20fishing%20of%20cod%20and%20haddock%202006.pdf](http://www.regjeringen.no/Upload/FKD/Vedlegg/Diverse/2007/Status%20report%20-%20Russian%20fishing%20of%20cod%20and%20haddock%202006.pdf) [Accessed 29 October 2013].
- Dutch Fish Product Board, 2008. *Plaice and Sole – An Overview of Activities and Measures in the Flatfish Sector*. Productschap VIS, Rijswijk, the Netherlands. Available from: [www.pvis.nl/fileadmin/user\\_upload/pvis/Documenten/Verantwoorde\\_vis/Plaice\\_and\\_sole\\_activities\\_and\\_measures.pdf](http://www.pvis.nl/fileadmin/user_upload/pvis/Documenten/Verantwoorde_vis/Plaice_and_sole_activities_and_measures.pdf) [Accessed 29 October 2013].
- Edwards, Paul N., 1996a. Global Comprehensive Models in Politics and Policymaking. *Climatic Change*, 32(2), pp.149–161.
- Edwards, P N, 1996b. *The Closed World: Computers and the Politics of Discourse in Cold War America*, Cambridge, Massachusetts: MIT Press.
- Edwards, P.N., 1999. Global Climate Science, Uncertainty and Politics: Data-Laden Models, Model-Filtered Data. *Science as Culture*, 8(4), pp.437–472.



- Edwards, P.N., 2001. Representing the Global Atmosphere: Computer Models, Data, and Knowledge about Climate Change. In C. Miller & P. N. Edwards, eds. *Changing the Atmosphere: Expert Knowledge and Environmental Governance*. Cambridge, Massachusetts: MIT Press, pp. 31–65.
- Edwards, P.N., 2010. *A Vast Machine*, Cambridge, Massachusetts: MIT Press.
- Edwards, P.N., 2012. Entangled Histories: Climate Science and Nuclear Weapons Research. *Bulletin of the Atomic Scientists*, 68(4), pp.28–40.
- Eide, A. et al., 2003. Harvest Functions: The Norwegian Bottom Trawl Cod Fisheries. *Marine Resource Economics*, 18(1), pp.81–93.
- England, K.V.L., 1994. Getting Personal: Reflexivity, Positionality, and Feminist Research. *The Professional Geographer*, 46(1), pp.80–89.
- European Commission, 2000. *Communication from the Commission to the Council and the European Parliament – Application of the Precautionary Principle and Multiannual Arrangements for Setting TACs*. COM(2000) 803 final. Commission of the European Communities, Brussels, Belgium. 01.12.2000.
- European Commission, 2001a. *Communication from the Commission – A Sustainable Europe for a Better World A European Union Strategy for Sustainable Development (Commission's proposal to the Gothenburg European Council)*. COM(2001) 264 final. Commission of the European Communities. Brussels, Belgium. 15.05.2001
- European Commission, 2001b. *European Governance – A White Paper*. COM(2001) 428 final. Commission of the European Communities, Brussels, Belgium. 25.07.2001.
- European Commission, 2001c. *Green Paper on the Future of the Common Fisheries Policy*. COM(2001) 135 final. Commission of the European Communities, Brussels, Belgium. 20.03.2001.
- European Commission, 2002a. *Communication from the Commission Action Plan – Simplifying and Improving the Regulatory Environment*. COM(2002) 278 final. Commission of the European Communities, Brussels, Belgium. 05.06.2002.
- European Commission, 2002b. *Communication from the Commission on Impact Assessment*. COM(2002) 276 final. Commission of the European Communities, Brussels, Belgium. 05.06.2002.

- European Commission, 2002c. *Communication from the Commission on the Collection and Use of Expertise by the Commission: Principle and Guidelines – Improving The Knowledge Base for Better Policies*. COM(2002) 713 final. Commission of the European Communities, Brussels, Belgium. 11.12.2002.
- European Commission, 2002d. *Communication from the Commission – Towards a Reinforced Culture of Consultation and Dialogue – General Principles and Minimum Standards for Consultation of Interested Parties by the Commission*. COM(2002) 704 final. Commission of the European Communities, Brussels, Belgium. 11.12.2002.
- European Commission, 2005a. *Impact Assessment Guidelines*. SEC(2005) 791. Commission of the European Communities, Brussels, Belgium. 15.06.2005.
- European Commission, 2005b. *Proposal for a Council Regulation – Establishing a Management Plan for Fisheries Exploiting Stocks of Plaice and Sole in the North Sea*. COM(2005) 714 final 2006/0002 (CNS). Commission of the European Communities, Brussels, Belgium. 10.01.2006.
- European Commission, 2005c. Regional Advisory Councils: Involving Stakeholders in the Decision-Making Process. *Fishing in Europe*, No 25, pp.4–8 [online]. Available from:  
[http://ec.europa.eu/fisheries/documentation/magazine/mag25\\_en.pdf](http://ec.europa.eu/fisheries/documentation/magazine/mag25_en.pdf) [Accessed 29 October 2013].
- European Commission, 2006a. *Communication from the Commission to the Council and the European Parliament – Implementing Sustainability in EU Fisheries through Maximum Sustainable Yield*. COM(2006) 360 final. Commission of the European Communities, Brussels, Belgium. 04.07.2006.
- European Commission (2006b). *Commission Staff Working Paper, Report of the STECF Working Group on Impact assessment: Plaice and sole Long-term Management*. SGECA-SGRST-06-05. Commission of the European Communities, Scientific, Technical and Economic Committee for Fisheries, Brussels, Belgium.
- European Commission, 2006c. *Commission Staff Working Paper, 23rd Report of the Scientific, Technical and Economic Committee for Fisheries (Second Plenary Meeting) – Ispra, 6-10 November 2006*. Advanced copy pending SEC number. Commission of the European Communities, Scientific, Technical and Economic Committee for Fisheries, Brussels, Belgium.

- European Commission, 2007. *Commission Staff Working Paper, 24th Report of the Scientific, Technical and Economic Committee for Fisheries (Spring Plenary Meeting) – Ispra, 23-27 April 2007*. Pending SEC number. Commission of the European Communities, Scientific, Technical and Economic Committee for Fisheries, Brussels, Belgium.
- European Commission, 2013a. *Reform Package* [online]. Available from: [http://ec.europa.eu/fisheries/reform/proposals/index\\_en.htm](http://ec.europa.eu/fisheries/reform/proposals/index_en.htm) [Accessed 29 October 2013].
- European Commission, 2013b. *The Common Fisheries Policy (CFP)* [online]. Available from: [http://ec.europa.eu/fisheries/cfp/index\\_en.htm](http://ec.europa.eu/fisheries/cfp/index_en.htm) [Accessed 29 October 2013].
- European Council, 1998. Cardiff European Council of 15 and 16 June 1988 – *Presidency Conclusions*. SN 150/1/98 REV 1. European Council, Brussels.
- European Parliament, 2006. *European Parliament Legislative Resolution on the Proposal for a Council Regulation Establishing a Management Plan for Fisheries Exploiting Stocks of Plaice and Sole in the North Sea*. (COM(2005)0714 – C60034/2006 – 2006/0002(CNS)) FINAL A6-0265/2006. European Parliament, Committee on Fisheries, [s.l.]. 31.08.2006.
- Evans, R., 1999. *Macroeconomic Forecasting: A Sociological Appraisal*, London: Routledge.
- Ezrahi, Y., 1990. *The Descent of Icarus Science and the Transformation of Contemporary Democracy*, Cambridge, Massachusetts: Harvard University Press.
- Ezrahi, Y., 2004. Science and the Political Imagination in Contemporary Democracies. In S. Jasanoff, ed. *States of Knowledge: The Co-Production of Science and Social Order*. London: Routledge, pp. 254–273.
- FAO, 2003. Fisheries Management: The Ecosystem Approach to Fisheries. *Technical Guidelines for Responsible Fisheries Management*, 4(2), pp.1–121.
- FAO, 2013. *Aquatic Species Fact Sheets* [online]. Available from: [www.fao.org/fishery/species/search/en](http://www.fao.org/fishery/species/search/en) [Accessed 29 October 2013].
- Farnell, J. & Elles, J., 1984. *In Search of a Common Fisheries Policy*, Aldershot: Gower.

- Farrell, A. & Keating, T.J., 2005. Dissent and Trust in Assessment Processes: A Comparison of OTAG and LRTAP. In A. Farrell & J. Jäger, eds. *Assessments of Regional and Global Environmental Risks: Designing Processes for the Effective Use of Science in Decisionmaking*. Washington D.C.: RFF Press, pp. 64–83.
- Farrell, A., VanDeveer, S.D. & Jäger, J., 2001. Environmental Assessments: Four Under-Appreciated Elements of Design. *Global Environmental Change*, 11(4), pp.311–333.
- Felt, U. & Fochler, M., 2010. Machineries for Making Publics: Inscribing and Describing Publics in Public Engagement. *Minerva*, 48(3), pp.219–238.
- Finlayson, A., 1994. *Fishing for Truth: A Sociological Analysis of Northern Cod Stock Assessments from 1977-90*, St John's, Newfoundland: Memorial University of Newfoundland.
- Fleck, J., 1993. Configurations: Crystallizing Contingency. *International Journal of Human Factors in Manufacturing*, 3(1), pp.15–36.
- Flyvbjerg, B., 2006. Five Misunderstandings About Case-Study Research. *Qualitative Inquiry*, 12(2), pp.219–245.
- Foucault, M., 2007. *Security, Territory, Population*, translated by Burchell, G., Basingstoke: Palgrave Macmillan.
- Fujimura, J.H., 1992. Crafting Science: Standardized Packages, Boundary Objects, and “Translation”. In A. Pickering, ed. *Science as Practice and Culture*. Chicago, Illinois: University of Chicago Press, pp. 168–211.
- Funtowicz, S.O. & Ravetz, J.R., 1993. Science for the Post-Normal Age. *Futures*, 25(7), pp.739–755.
- Galison, P., 1996. Computer Simulations and the Trading Zone. In P. Galison & D. J. Stump, eds. *The Disunity of Science: Boundaries, Contexts, and Power*. Stanford, California: Stanford University Press, pp. 118–157.
- Galison, P., 1997. *Image and Logic: A Material Culture of Microphysics*, Chicago, Illinois: University of Chicago Press.
- Garcia, S.M., 1992. Ocean Fisheries Management: The FAO Programme. In P. Fabbri, ed. *Ocean Management in Global Change*. Barking: Elsevier, pp. 381–418.

- Geels, F.W., 2007. Feelings of Discontent and the Promise of Middle Range Theory for STS: Examples from Technology Dynamics. *Science, Technology & Human Values*, 32(6), pp.627–651.
- Gibbons, M. et al., 1994. *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies*, London: Sage.
- Gieryn, T.F., 1983. Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists. *American Sociological Review*, 48(6), pp.781–795.
- Gieryn, T.F., 1995. Boundaries of Science. In S. Jasanoff et al., eds. *Handbook of Science and Technology Studies*. Thousand Oaks, California: Sage, pp. 393–443.
- Glaser, B. & Strauss, A., 1967. *The Discovery of Grounded Theory*, Chicago: Aldine.
- Goffman, E., 1961. *Encounters: Two Studies in the Sociology of Interaction*, Indianapolis: Bobbs-Merrill.
- Goffman, E., 1974. *Frame Analysis: An Essay on the Organization of Experience*, Cambridge, Massachusetts: Harvard University Press.
- Goodman, L.A., 1961. Snowball Sampling. *The Annals of Mathematical Statistics*, 32(1), pp.148–170.
- Gough, C. & Shackley, S., 2002. The Use of Integrated Assessment: An Institutional Analysis Perspective. *Tyndall Centre Working Papers*, (14). Tyndall Centre, Norwich, UK. Available from: [www.tyndall.ac.uk/content/use-integrated-assessment-institutional-analysis-perspective](http://www.tyndall.ac.uk/content/use-integrated-assessment-institutional-analysis-perspective) [Accessed 29 October 2013].
- Gough, C., Castells, N. & Funtowicz, S., 1998. Integrated Assessment: An Emerging Methodology for Complex Issues. *Environmental Modelling and Assessment*, 3(1-2), pp.19–29.
- Gray, T.S. ed., 2005. *Participation in Fisheries Governance*, Dordrecht, the Netherlands: Springer.
- Gray, T.S. & Hatchard, J., 2003. The 2002 Reform of the Common Fisheries Policy's System of Governance – Rhetoric or Reality? *Marine Policy*, 27(6), pp.545–554.
- Gray, T.S. & Hatchard, J., 2007. Environmental Stewardship as a New Form of Fisheries Governance. *ICES Journal of Marine Science*, 64(4), pp.786–792.

- Griffin, L., 2007. All Aboard: Power, Participation and Governance in the North Sea Regional Advisory Council. *International Journal of Green Economics*, 1(3/4), pp.478–493.
- Griffin, L., 2009. Scales of Knowledge: North Sea Fisheries Governance, the Local Fisherman and the European Scientist. *Environmental Politics*, 18(4), pp.557–575.
- Griffin, L., 2010. The Limits to Good Governance and the State of Exception: A Case Study of North Sea Fisheries. *Geoforum*, 41(2), pp.282–292.
- Griffin, L., 2013. *Good Governance, Scale and Power: A Case Study of North Sea Fisheries*, Oxford: Routledge.
- Griff, R.E. et al., 2005. *Evaluation of Management Measures for a Sustainable Plaice Fishery in the North Sea*. RIVO-Netherlands Institute for Fisheries Research, C019/05, Ijmuiden, the Netherlands. Available from: <http://edepot.wur.nl/21640> [Accessed 29 October 2013].
- Groeneveld, R.A., 2011. Quantifying Fishers' and Citizens' Support for Dutch Flatfish Management Policy. *ICES Journal of Marine Science*, 68(5), pp.919–928.
- Gullestad, P. et al., 2013. Changing Attitudes 1970-2012: Evolution of the Norwegian Management Framework to Prevent Overfishing and to Secure Long-term Sustainability. *ICES Journal of Marine Science* [online]. Available from: <http://icesjms.oxfordjournals.org/cgi/doi/10.1093/icesjms/fst094> [Accessed 29 October 2013].
- Guston, D.H., 2001. Boundary Organizations in Environmental Policy and Science: An Introduction. *Science, Technology & Human Values*, 26(4), pp.399–408.
- Haas, P.M., 1992. Introduction: Epistemic Communities and International Policy Coordination. *International Organization*, 46(1), pp.1–35.
- Haas, P.M., 1997. Scientific Communities and Multiple Paths to Environmental Management. In L. A. Brooks & S. D. VanDeveer, eds. *Saving the Seas: Values, Scientists, and International Governance*. College Park: Maryland Sea Grant College, pp. 193–228.
- Hacking, I., 1999. *The Social Construction of What*, Cambridge, Massachusetts: Harvard University Press.
- Hajer, M.A., 1995. *The Politics of Environmental Discourse: Ecological Modernization and the Policy Process*, Oxford: Clarendon Press.

- Halfman, W., 2003. *Boundaries of Regulatory Science: Eco/toxicology and the Regulation of Aquatic Hazards of Chemicals in the US, England, and the Netherlands, 1970-1995*. Thesis (PhD). University of Amsterdam. Synopsis available from: <http://halfman.net/papers/th%20synopsis.pdf> [Accessed 29 October 2013]
- Halfman, W., 2008. *States of Nature: Nature and Fish Stock Reports for Policy*, The Hague, the Netherlands: RMNO, Netherlands Council for Research on Spatial Planning, Nature and the Environment.
- Halfman, W. & Hoppe, R., 2005. Science/Policy Boundaries: A Changing Division of Labour in Dutch Expert Policy Advice. *Sociology of the Sciences Yearbook*, 24, pp.135–151.
- Halpern, C., 2010. Governing Despite its Instruments? Instrumentation in EU Environmental Policy. *West European Politics*, 33(1), pp.39–57.
- Hammer, M. & Hoel, A.H., 2012. The Development of Scientific Cooperation under the Norway – Russia Fisheries Regime in the Barents Sea. *Arctic Review on Law and Politics*, 3(2), pp.244–274.
- Hammersley, M. & Atkinson, P., 1995. *Ethnography: Principles in Practice*, London: Routledge.
- Haraway, D., 1991. *Simians, Cyborgs, and Women: The Reinvention of Women*, London: Routledge.
- Hardin, G., 1968. The Tragedy of the Commons. *Science*, 162(3859), pp.1243–1248.
- Hartmann, S., 1996. The World as a Process: Simulations in the Natural and the Social Sciences. In R. Hegselmann, U. Mueller, & K. G. Troitzsch, eds. *Modelling and Simulation in the Social Sciences from the Philosophy of Science Point of View*. Dordrecht, the Netherlands: Kluwer, pp. 77–100.
- Hawkins, A.D., 2005. The Role of Partnerships in the Governance of Fisheries within the European Union Participation in Fisheries Governance. In T. S. Gray, ed. *Participation in Fisheries Governance*. Dordrecht, the Netherlands: Springer, pp. 65–83.
- Hegland, T.J. & Wilson, D.C., 2009. Participatory Modelling in EU Fisheries Management: Western Horse Mackerel and the Pelagic RAC. *Maritime Studies*, 8(1), pp.75–96.

- Hellenic Presidency of the Council of the European Union, 2003. *Report to the Ministers Responsible for Public Administration in the EU member States on the Progress of the Implementation of the Mandelkern Report's Action Plan on Better Regulation*. Hellenic Presidency of the Council of the European Union, Athens, Greece.
- Hersoug, B., 2005. *Closing the Commons: Norwegian Fisheries from Open Access to Private Property*, Delft, the Netherlands: Eburon.
- Hertin, J et al., 2009a. Rationalising the Policy Mess? Ex Ante Policy Assessment and the Utilisation of Knowledge in the Policy Process. *Environment and Planning A*, 41(5), pp.1185–1200.
- Hertin, J. et al., 2009b. The Production and Use of Knowledge in Regulatory Impact Assessment – An Empirical Analysis. *Forest Policy and Economics*, 11(5-6), pp.413–421.
- Hess, D., 2001. Ethnography and the Development of Science and Technology Studies. In P. Atkinson et al., eds. *Handbook of Ethnography*. London: Sage, pp. 234–245.
- Higginson, C., 1993. The UN Conference on High Seas Fishing. *Review of European Community & International Environmental Law*, 2(3), pp.237–243.
- Hilborn, R. & Walters, C., 1992. *Quantitative Fisheries Stock Assessment: Choice, Dynamics, and Uncertainty*, Dordrecht, the Netherlands: Kluwer.
- Holden, M., 1994. *The Common Fisheries Policy: Origin, Evaluation and Future*, Fishing News Books.
- Holm, P., 1996. Fisheries Management and the Domestication of Nature. *Sociologia Ruralis*, 36(2), pp.177–188.
- Holm, P., 1999. Fisheries Resource Management as a Heterogeneous Network. In: St. Francis Xavier University, *Ecological Knowledge Working Seminar*. Antigonish, Nova Scotia, Canada, May 24-31 1999.
- Holm, P., 2001. *The Invisible Revolution: The Construction of Institutional Change in the Fisheries*. Thesis (PhD). University of Tromsø.
- Holm, P., 2003. Crossing the Border: On the Relationship Between Science and Fishermen's Knowledge in a Resource Management Context. *Maritime Studies*, 2(1), pp.5–33.



- Holm, P., 2007. Which Way is Up on Callon? In D. A. MacKenzie, F. Muniesa, & L. Siu, eds. *Do Economists Make Markets?: On the Performativity of Economics*. Princeton, New Jersey: Princeton University Press, pp. 225–243.
- Holm, P. & Nielsen, K.N., 2004. The TAC Machine. In: *Report of the Working Group on Fishery Systems, WD1 (Annex B). ICES CM 2004/D:06*. Copenhagen: ICES.
- Holm, P. & Nielsen, K.N., 2007. Framing Fish, Making Markets: The Construction of Individual Transferable Quotas (ITQs). In M. Callon, Y. Millo, & Fabian Muniesa, eds. *Market Devices*. Oxford: Blackwell, pp. 173–195.
- Holm, P. et al., 2009. *Nordic Experience of Fisheries Management. Seen in Relation to the Reform of the EU Common Fisheries*. Norden-Nordic Council of Ministers, TemaNord 2009:579, Copenhagen, Denmark. Available from: <http://munin.uit.no/bitstream/handle/10037/2462/article.pdf?sequence=1> [Accessed 29 October 2013].
- Hønneland, G., 2004. Fish Discourse: Russia, Norway, and the Northeast Arctic Cod. *Human Organization*, 63(1), pp.68–77.
- Hønneland, G., 2005. Fisheries Management in Post-Soviet Russia: Legislation, Principles, and Structure. *Ocean Development & International Law*, 36(2), pp.179–194.
- Hønneland, G., 2007. Norway and Russia in the Barents Sea – Cooperation and Conflict in Fisheries Management. *Russian Analytical Digest*, (20), pp.9–11.
- Hønneland, G., 2012. *Making Fishery Agreements Work: Post-agreement Bargaining in the Barents Sea*, Cheltenham: Edward Elgar.
- Hønneland, G. & Jørgensen, A.-K., 2002. Implementing International Fisheries Agreements in Russia – Lessons from the Northern Basin. *Marine Policy*, 26(5), pp.359–367.
- Hoppe, R., 1999. Policy Analysis, Science and Politics: From “Speaking Truth to Power” to “Making Sense Together”. *Science and Public Policy*, 26(3), pp.201–210.
- Hordijk, L., 1991. Use of the RAINS Model in Acid Rain Negotiations in Europe. *Environmental Science & Technology*, 25(4), pp.596–603.
- Hunt, J. & Shackley, S., 1999. Reconceiving Science and Policy: Academic, Fiducial and Bureaucratic Knowledge. *Minerva*, 37(2), pp.141–164.

- ICES, 1998. *Report of the Study Group on the Precautionary Approach to Fisheries Management*, Copenhagen, Denmark, 3-6 February 1998. ICES CM 1998/ACFM:10. International Council for the Exploration of the Sea, Study Group on the Precautionary Approach to Fisheries Management
- ICES, 2004a. *Report of the Arctic Fisheries Working Group (AFWG)*, Copenhagen, Denmark, 19-28 April 2006. ICES CM 2004/ACFM:28. International Council for the Exploration of the Sea, Arctic Fisheries Working Group.
- ICES, 2004b. *Report of the ICES Advisory Committee on Fishery Management and Advisory Committee on Ecosystems, 2004*. ICES Advice, 2004. Volume 1, Number 2. International Council for the Exploration of the Seas, Advisory Committee on Fishery Management and Advisory Committee on Ecosystems, Copenhagen, Denmark.
- ICES, 2005. *Report of the Arctic Fisheries Working Group (AFWG)*, Murmansk, Russia, 19-28 April 2005. ICES CM 2005/ACFM:20. International Council for the Exploration of the Sea, Arctic Fisheries Working Group.
- ICES, 2006. *Report of the Arctic Fisheries Working Group (AFWG)*, Copenhagen, Denmark, 19-28 April 2006. ICES CM 2006/ACFM:25. International Council for the Exploration of the Sea, Arctic Fisheries Working Group.
- ICES, 2007a. *Fish Stocks: Counting the Uncountable?* [leaflet]. Available from: <http://info.ices.dk/products/misc.asp> [Accessed 29 October 2013].
- ICES, 2007b. *Report of the Arctic Fisheries Working Group (AFWG)*, Vigo, Spain, 18-27 April 2007. ICES CM 2007/ACFM:16. International Council for the Exploration of the Sea, Arctic Fisheries Working Group.
- ICES, 2009. *Report of the ICES Advisory Committee, 2009*. ICES Advice, 2009. Book 3. International Council for the Exploration of the Seas, Copenhagen, Denmark.
- IMR, 2009. *Larger Cod Quota in the Barents Sea* [online]. Available from: [http://www.imr.no/nyhetsarkiv/2009/august/foreslar\\_storre\\_torskekvotar\\_i\\_barentshavet/en](http://www.imr.no/nyhetsarkiv/2009/august/foreslar_storre_torskekvotar_i_barentshavet/en) [Accessed 29 October 2013].
- Ingimundarson, V., 2003. Fighting the Cod Wars in the Cold War: Iceland's Challenge to the Western Alliance in the 1970s. *The RUSI Journal*, 148(3), pp.88–94.
- Ishii, A., 2011. Scientists Learn Not Only Science but Also Diplomacy: Learning Processes in the European Transboundary Air Pollution Regime. In R. Lidskog & G. Sundqvist, eds. *Governing the Air: The Dynamics of Science, Policy, and Citizen Interaction*. Cambridge, Massachusetts: The MIT Press, pp. 163–194.

- Ivanova, M., 2005. *The Northern Fisheries of the Russian Federation: Institutions in Transition*. Thesis (MSc). University of Tromsø. Available from: <http://munin.uit.no/bitstream/handle/10037/318/thesis.pdf?sequence=1> [Accessed 29 October 2013].
- Jacob, K. et al., 2008. *Improving the Practice of Impact Assessment*. Evaluating Integrated Impact Assessments (EVIA), CEC 6<sup>th</sup> Framework Programme, Project No. 028889. Available from: [http://userpage.fu-berlin.de/ffu/evia/EVIA\\_Policy\\_Paper.pdf](http://userpage.fu-berlin.de/ffu/evia/EVIA_Policy_Paper.pdf) [Accessed 29 October 2013].
- Jasanoff, S., 1990. *The Fifth Branch: Science Advisers as Policymakers*, Cambridge, Massachusetts: Harvard University Press.
- Jasanoff, S., 1995. *Science at the Bar: Law, Science, and Technology in America*, Cambridge, Massachusetts: Harvard University Press
- Jasanoff, S., 2003a. Breaking the Waves in Science Studies: Comment on HM Collins and Robert Evans, "The Third Wave of Science Studies." *Social Studies of Science*, 33(3), pp.389–400.
- Jasanoff, S., 2003b. Technologies of Humility: Citizen Participation in Governing Science. *Minerva*, 41(3), pp.223–244.
- Jasanoff, S., 2004. *States of Knowledge: The Co-Production of Science and Social Order*, London: Routledge.
- Jasanoff, S., 2005. *Designs on Nature: Science and Democracy in Europe and the United States*, Princeton, New Jersey: Princeton University Press.
- Jasanoff, S., 2010a. A Field of its Own: The Emergence of Science and Technology Studies. In *Oxford Handbook of Interdisciplinarity*. New York: Oxford University Press.
- Jasanoff, S., 2010b. A New Climate for Society. *Theory, Culture & Society*, 27(2-3), pp.233–253.
- Jasanoff, S., 2012. Genealogies of STS. *Social Studies of Science*, 43(2), pp.435–441.
- Jasanoff, S. & Wynne, B., 1998. Science and Decisionmaking. In S. Jasanoff & B. Wynne, eds. *Human Choice and Climate Change: An International Assessment*. Columbus, Ohio: Battelle Press, pp. 1–87.

- Jensen, C.B., 2013. Continuous Variations: The Conceptual and the Empirical in STS. *Science, Technology & Human Values* [online]. Available from: <http://sth.sagepub.com/content/early/2013/09/09/0162243913502866> [Accessed September 19 2013].
- Jentoft, S., 2004. The Community in Fisheries Management: Experiences, Opportunities and Risks. In B. Hersoug, S. Jentoft, & P. Degnbol, eds. *Fisheries Development: The Institutional Challenge*. Delft, the Netherlands: Eburon, pp. 93–129.
- Jentoft, S., 2005. Institutions for Fisheries Governance: Introduction. In P. Kooiman et al., eds. *Fish for Life: Interactive Governance for Fisheries*. Amsterdam: Amsterdam University Press, pp. 147–151.
- Jentoft, S., 2007. Limits of Governability: Institutional Implications for Fisheries and Coastal Governance. *Marine Policy*, 31(4), pp.360–370.
- Jentoft, S. & Mikalsen, K.H., 2004. A Vicious Circle? The Dynamics of Rule-Making in Norwegian Fisheries. *Marine Policy*, 28(2), pp.127–135.
- Jentoft, S., McCay, B.J. & Wilson, D.C., 1998. Social Theory and Fisheries Co-Management. *Marine policy*, 22(4), pp.423–436.
- Joerges, B., 1999. Do Politics Have Artefacts? *Social Studies of Science*, 29(3), pp.411–431.
- Johnsen, J.P., 2013. Is Fisheries Governance Possible? *Fish and Fisheries* [online]. Available from: <http://dx.doi.org/10.1111/faf.12024> [Accessed 29 October 2013].
- Johnsen, J.P. et al., 2009. The Cyborgization of the Fisheries: On Attempts to Make Fisheries Management Possible. *Maritime Studies*, 7(2), pp.9–34.
- Jorgensen, U. & Sorensen, O.H., 1999. Arenas of Development – A Space Populated by Actor-worlds, Artefacts, and Surprises. *Technology Analysis & Strategic Management*, 11(3), pp.409–429.
- Jullien, B. & Smith, A. eds., 2008. *Industries and Globalization: The Political Causality of Difference*, Basingstoke: Palgrave Macmillan.
- Kell, L. & Fromentin, J.M., 2006. *The Illusion of MSY*. ICES CM 2006/R:14. International Council for the Exploration of the Sea, Copenhagen, Denmark.
- Knorr-Cetina, K., 1997. Sociality with Objects: Social Relations in Postsocial Knowledge Societies. *Theory Culture and Society*, 14(4), pp.1–30.

- Knuuttila, T., 2002. Signing for Reflexivity: Constructionist Rhetorics and Its Reflexive Critique in Science and Technology Studies. *Forum: Qualitative Social Research*, 3(3) [online]. Available from: [www.qualitative-research.net/index.php/fqs/article/view/828/1798](http://www.qualitative-research.net/index.php/fqs/article/view/828/1798) [Accessed 29 October 2013].
- Knuuttila, T., 2005a. Models as Epistemic Artefacts: Toward a Non-Representationalist Account of Scientific Representation. *Philosophical Studies from the University of Helsinki*, 8 [online]. Available from: [www.helsinki.fi/cradle/documents/Doctoral dissertations/Knuuttila 2005 Models as Epistemic Artefacts.pdf](http://www.helsinki.fi/cradle/documents/Doctoral%20dissertations/Knuuttila%202005%20Models%20as%20Epistemic%20Artefacts.pdf) [Accessed 29 October 2013].
- Knuuttila, T., 2005b. Models, Representation, and Mediation. *Philosophy of Science*, 72(5), pp.1260–1271.
- Kooiman, J. et al. eds., 2005. *Fish for Life: Interactive Governance for Fisheries*, Amsterdam: Amsterdam University Press.
- Kraak, S.B.M. et al., 2010. On Scientists' Discomfort in Fisheries Advisory Science: The Example of Simulation-based Fisheries Management-Strategy Evaluations. *Fish and Fisheries*, 11(2), pp.119–132.
- Kuhn, T.S., 1970[1962]. *The Structure of Scientific Revolutions* 2nd ed., Chicago, Illinois: Chicago University Press.
- Kvale, S., 1996. *Interviews: An Introduction to Qualitative Research Interviewing*, London: Sage.
- Kwa, C., 1993. Modeling the Grasslands. *Historical Studies in the Physical and Biological Sciences*, 24(1), pp.125–155.
- Kwa, C., 1994. Modelling Technologies of Control. *Science as Culture*, 4(3), pp.363–391.
- Lagroye, J., 1994. Être du Métier. *Politix*, 7(28), pp.5–15.
- Lahsen, M., 2005. Seductive Simulations? Uncertainty Distribution Around Climate Models. *Social Studies of Science*, 35(6), pp.895–922.
- Landström, C. et al., 2011. Coproducing Flood Risk Knowledge: Redistributing Expertise in Critical “Participatory Modelling.” *Environment and Planning A*, 43(7), pp.1617–1633.
- Lane, S.N., Landström, C. & Whatmore, S.J., 2011. Imagining Flood Futures: Risk Assessment and Management in Practice. *Philosophical transactions. Series A, Mathematical, Physical, and Engineering Sciences*, 369(1942), pp.1784–806.

- Larkin, P.A., 1977. An Epitaph for the Concept of Maximum Sustained Yield. *Transactions of the American Fisheries Society*, 106(1), pp.1–11.
- Lascoumes, P. & Le Galès, P., 2007. Introduction: Understanding Public Policy through Its Instruments – From the Nature of Instruments to the Sociology of Public Policy Instrumentation. *Governance*, 20(1), pp.1–21.
- Lassen, H., Kelly, C. & Sissenwine, M., 2013. ICES Advisory Framework 1977–2012: From Fmax to Precautionary Approach and Beyond. *ICES Journal of Marine Science* [online]. Available from: <http://dx.doi.org/10.1093/icesjms/fst146> [Accessed 29 October 2013].
- Latour, B., 1987. *Science in Action: How to Follow Scientists and Engineers Through Society*, Cambridge, Massachusetts: Harvard University Press.
- Latour, B., 1992. One More Turn after the Social Turn: Easing Science Studies into the Non-Modern World. In E. McMullin, ed. *The Social Dimensions of Science*. Notre Dame: Indiana University of Notre Dame University Press, pp. 272–294.
- Latour, B., 1993. *We Have Never Been Modern*, Cambridge, Massachusetts: Harvard University Press.
- Latour, B., 1999. On Recalling ANT. In J. Law & J. Hassard, eds. *Actor Network Theory and After*. Oxford: Blackwell Oxford, pp. 93–95.
- Latour, B., 2003. Is Re-modernization Occurring – And If So, How to Prove It? A Commentary on Ulrich Beck. *Theory, Culture & Society*, 20(2), pp.35–48.
- Latour, B., 2005. *Reassembling the Social: An Introduction to Actor-Network Theory*, Oxford: Oxford University Press.
- Latour, B. & Woolgar, S., 1986. *Laboratory of Life: The Construction of Scientific Facts*, Princeton, New Jersey: Princeton University.
- Law, J., 1987. Technology and Heterogeneous Engineering: The Case of Portuguese Expansion. In W. E. Bijker, T. Hughes, & T. Pinch, eds. *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*. Cambridge, Massachusetts: MIT Press, pp. 111–134.
- Law, J., 2006. Disaster in Agriculture: Or Foot and Mouth Mobilities. *Environment and Planning A*, 38(2), pp.227–239.
- Law, J., 2008. Culling, Catastrophe and Collectivity. *Distinktion: Scandinavian Journal of Social Theory* (16), pp.61–76.

- Law, J., 2011. *Heterogeneous Engineering and Tinkering* [online]. Available from: [www.heterogeneities.net/publications/Law2011HeterogeneousEngineeringAndTinkering.pdf](http://www.heterogeneities.net/publications/Law2011HeterogeneousEngineeringAndTinkering.pdf) [Accessed 29 October 2013].
- Law, J., 2012. *Notes on Fish, Ponds and Theory*. Newcomers to the Farm Project. The Norwegian Research Council, Project No. 183352/S30. Available from: [www.sv.uio.no/sai/english/research/projects/newcomers/publications/working-papers-web/law-notes-on-fish-ponds-and-theory.pdf](http://www.sv.uio.no/sai/english/research/projects/newcomers/publications/working-papers-web/law-notes-on-fish-ponds-and-theory.pdf) [Accessed 29 October 2013].
- Law, J. & Hassard, J. eds., 1999. *Actor Network Theory and After*, Oxford: Blackwell.
- Lequesne, C., 2004. *The Politics of Fisheries in the European Union*, Manchester: Manchester University Press.
- Lidskog, R. & Sundqvist, G., 2002. The Role of Science in Environmental Regimes: The Case of LRTAP. *European Journal of International Relations*, 8(1), pp.77–101.
- Lidskog, R. & Sundqvist, G. eds., 2011. *Governing the Air: The Dynamics of Science, Policy, and Citizen Interaction*, Cambridge, Massachusetts: MIT Press.
- Lie, M. & Sørensen, K.H. eds., 1996. *Making Technology Our Own?: Domesticating Technology Into Everyday Life*, Oslo: Scandinavian University Press.
- Linke, S. & Jentoft, S., 2013. A Communicative Turnaround: Shifting the Burden of Proof in European Fisheries Governance. *Marine Policy*, 38(0), pp.337–345.
- Linke, S., Dreyer, M. & Sellke, P., 2011. The Regional Advisory Councils: What is Their Potential to Incorporate Stakeholder Knowledge into Fisheries Governance? *AMBIO*, 40(2), pp.133–143.
- Livingston, E., 1999. Cultures of Proving. *Social Studies of Science*, 29(6), pp.867–888.
- Lynch, M., 1997. *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science*, Cambridge: Cambridge University Press.
- Lynch, M., 2001. A Pragmatogony of Factishes. *Metascience: An International Journal for the History, Philosophy and Social Studies of Science*, 10(2), pp.223–231.
- Lynch, M., 2012. Self-exemplifying Revolutions? Notes on Kuhn and Latour. *Social Studies of Science*, 42(3), pp.449–455.



- Maas, R. et al., 2004. Integrated Assessment Modelling – The Tool. In J. Sliggers & W. Kakebeeke, eds. *Clearing the Air: 25 Years of the Convention on Long-range Transboundary Air Pollution*. New York and Geneva: United Nations. Economic Commission for Europe, pp. 85–96.
- Maasen, S. & Weingart, P. eds., 2006. *Democratization of Expertise?: Exploring Novel Forms of Scientific Advice in Political Decision-Making*, Dordrecht, the Netherlands: Springer.
- Mackenzie, D., 1990. *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidelines*, Cambridge, Massachusetts: MIT Press.
- MacKenzie, D., 1998. *Knowing Machines: Essays on Technical Change*, Cambridge, Massachusetts: MIT Press.
- MacKenzie, D., 2003. An Equation and its Worlds: Bricolage, Exemplars, Disunity and Performativity in Financial Economics . *Social Studies of Science* , 33 (6 ), pp.831–868.
- MacKenzie, D., 2005. Opening the Black Boxes of Global Finance. *Review of International Political Economy*, 12(4), pp.555–576.
- MacKenzie, D., 2006. *An Engine, Not a Camera: How Financial Models Shape Markets*, Cambridge, Massachusetts: MIT Press.
- MacKenzie, D., 2009. Making Things the Same: Gases, Emission Rights and the Politics of Carbon Markets. *Accounting, Organizations and Society*, 34(3–4), pp.440–455.
- MacKenzie, D. & Wajcman, J. eds., 1999[1985]. *The Social Shaping of Technology* 2nd ed., Buckingham: Open University Press.
- Marcus, G.E., 1995. Ethnography in/of the World System: The Emergence of Multi-Sited Ethnography. *Annual Review of Anthropology*, 24, pp.95–117.
- Mattila, E., 2005. Interdisciplinarity “In the Making”: Modeling Infectious Diseases. *Perspectives on Science*, 13(4), pp.531–553.
- McGlade, J. ed., 1999. *Advanced Ecological Theory: Principles and Applications*, Malden: Blackwell.
- McCay, B.J. & Acheson, J.M. eds., 1987. *The Question of the Commons: The Culture and Ecology of Communal Resources*, Tucson: University of Arizona Press.



- Meadows, D.H. et al., 1972. *Limits to Growth: A Report for the Club of Rome's Project on the Predicament of Mankind*, New York: Universe Books.
- Meadows, D.H., Meadows, D.L. & Randers, J., 2004. *Limits to Growth: The 30-year Update*, White River Junction, Vermont: Chelsea Green.
- Merz, M., 1999. Multiplex and Unfolding: Computer Simulation in Particle Physics. *Science in Context*, 12(02), pp.293–316.
- Meuwese, A.C.M., 2008. *Impact Assessment in EU Lawmaking*. Thesis (PhD). University of Leiden. Available from: <https://openaccess.leidenuniv.nl/bitstream/handle/1887/12589/Thesis.pdf> [Accessed 29 October 2013].
- Miller, C.A. & Edwards, P. eds., 2001. *Changing the Atmosphere: Expert Knowledge and Environmental Governance*, Cambridge, Massachusetts: MIT Press.
- Miller, D., 2002. Turning Callon the Right Way Up. *Economy and Society*, 31(2), pp.218–233.
- Miller, P. & Rose, N., 1990. Governing Economic Life. *Economy and Society*, 19(1), pp.1–31.
- Millo, Y. & MacKenzie, D., 2007. Building a Boundary Object: The Evolution of Financial Risk Management. *CARR Discussion Papers*, DP 48. Centre for Analysis of Risk and Regulation, London School of Economics and Political Science, London, UK. Available from: <http://eprints.lse.ac.uk/36530> [Accessed 29 October 2013].
- Millo, Y. & MacKenzie, D., 2009. The Usefulness of Inaccurate Models: Towards an Understanding of the Emergence of Financial Risk Management. *Accounting, Organizations and Society*, 34(5), pp.638–653.
- Morgan, M.S. & Morrison, M. eds., 1999. *Models as Mediators: Perspectives on Natural and Social Science*, Cambridge: Cambridge University Press.
- Nielsen, K.N., 2008. *Boundary Construction in Mandated Science: The Case of ICES' Advice on Fisheries Management*. Thesis (PhD). University of Tromsø. Available from: <http://munin.uit.no/bitstream/handle/10037/2401/thesis.pdf?sequence=1> [Accessed 29 October 2013].
- Nielsen, K.N. & Holm, P., 2007. A Brief Catalogue of Failures: Framing Evaluation and Learning in Fisheries Resource Management. *Marine Policy*, 31(6), pp.669–680.

- Noble, D.F., 1978. Social Choice in Machine Design: The Case of Automatically Controlled Machine Tools, and a Challenge for Labor. *Politics & Society*, 8(3-4), pp.313–347.
- Nordstrand, K.B. & Holm, P., 2009. Refurbishing the Coast: The Invention of the Tourist-Fischer. *Maritime Studies*, 7(2), pp.83–97.
- Nowotny, H., Scott, P. & Gibbons, M., 2001. *Re-Thinking Science: Knowledge and the Public in an Age of Uncertainty*, Cambridge: Polity Press.
- NSRAC, 2004a. *Initial Advice to the European Commission Concerning Management Measures for North Sea plaice*. North Sea Regional Advisory Council, Aberdeen, UK. 12.11.04.
- NSRAC, 2004b. *Report of the North Sea Regional Advisory Council First General Assembly*, Edinburgh, UK, 4 November 2004. North Sea Regional Advisory Council, Aberdeen, UK.
- NSRAC, 2004c. *Report of the Preliminary Meeting of the Executive Committee of the North Sea Regional Advisory Council*, Edinburgh, UK, 5 November 2004. North Sea Regional Advisory Council, Aberdeen, UK.
- NSRAC, 2005a. *Flatfish Working Group Advice to the European Commission – Immediate Management Measures for North Sea Plaice (Final)*. North Sea Regional Advisory Council, Flatfish Working Group, Aberdeen, UK. 12.07.2005.
- NSRAC, 2005b. *Report of the First Meeting of the Executive Committee of the North Sea Regional Advisory Council*, Boulogne s/Mer, France, 31 January – 1 February 2005. North Sea Regional Advisory Council, Flatfish Working Group, Aberdeen, UK.
- NSRAC, 2005c. *Flatfish Working Group Agreed Record of Meeting for ExCom*, Brussels, Belgium, 15 November 2005. North Sea Regional Advisory Council, Flatfish Working Group, Aberdeen, UK.
- NSRAC, 2005d. *Flatfish Working Group Agreed Record of Meeting for ExCom*, Glasgow, UK, 18-19 May 2005. North Sea Regional Advisory Council, Flatfish Working Group, Aberdeen, UK.
- NSRAC, 2006a. *Demersal Working Group Agreed Record of Meeting*, Brussels, Belgium, 28 November 2006. North Sea Regional Advisory Council, Demersal Working Group, Aberdeen, UK.

- NSRAC, 2006b. *Executive Committee (ExCom) Record of Meeting*, Den Helder, the Netherlands, 16 June 2006. North Sea Regional Advisory Council, Aberdeen, UK.
- NSRAC, 2006c. *Flatfish Working Group Agreed Record of Meeting for ExCom*, Gdynia, Poland, 22 February 2006. North Sea Regional Advisory Council, Flatfish Working Group, Aberdeen, UK.
- NSRAC, 2006d. *Letter to Commissioner Borg*. North Sea Regional Advisory Council, Aberdeen, UK. 02.02.2006.
- NSRAC, 2007a. Advisory Committee Urges Commission to Re-think Flatfish Advice. [press release] 23 October 2007. Available from: [http://nsrac.org/wp-content/uploads/2009/09/20071023\\_Flatfish.pdf](http://nsrac.org/wp-content/uploads/2009/09/20071023_Flatfish.pdf) [Accessed 29 October 2013].
- NSRAC, 2007b. *Demersal Working Group Record of Meeting*, Brussels, Belgium, 16 November 2007. North Sea Regional Advisory Council, Demersal Working Group, Aberdeen, UK.
- NSRAC, 2007c. *Demersal Working Group Record of Meeting*, Copenhagen, Denmark, 11 April 2007. North Sea Regional Advisory Council, Demersal Working Group, Aberdeen, UK.
- NSRAC, 2007d. *Executive Committee (ExCom) Record of Meeting*, Vienna, Austria, 14 June 2007. North Sea Regional Advisory Council, Aberdeen, UK.
- NSRAC, 2007e. *Letter to the European Commission DG Fisheries, Mr Fokion Fotiadis Re ICES Advice on North Sea Plaice and Sole*. North Sea Regional Advisory Council, Aberdeen, UK. 18.10.2007.
- O’Leary, B.C. et al., 2011. Fisheries Mismanagement. *Marine Pollution Bulletin*, 62(12), pp.2642–2648.
- O’Reilly, J., Oreskes, N. & Oppenheimer, M., 2012. The Rapid Disintegration of Projections: The West Antarctic Ice Sheet and the Intergovernmental Panel on Climate Change. *Social Studies of Science*, 42(5), pp.709–731.
- Office of the Auditor General of Norway, 2007. *The Office of the Auditor General’s Investigation of the Management and Control of Fish Resources in the Barents Sea and the Norwegian Sea – A Parallel Audit Conducted by the Office of the Auditor General of Norway and the Accounts Chamber of the Russian*. Office of the Auditor General of Norway, 3:2 (2007-2008). Available from: [www.riksrevisjonen.no/en/SiteCollectionDocuments/Dokumentbasen/Engelsk/Document 3/Doc\\_3\\_2\\_2007\\_2008\\_eng.pdf](http://www.riksrevisjonen.no/en/SiteCollectionDocuments/Dokumentbasen/Engelsk/Document%203/Doc_3_2_2007_2008_eng.pdf) [Accessed 29 October 2013].

- Oreskes, N., Shrader-Frechette, K. & Belitz, K., 1994. Verification, Validation, and Confirmation of Numerical Models in the Earth Sciences. *Science*, 263(5147), pp.641–646.
- Owens, S., 2007. A Balanced Appraisal? Impact Assessment of European Commission Proposals. *Environmental Law Network International Review*, (1), pp.2–8.
- Owens, S., Rayner, T. & Bina, O., 2004. New Agendas for Appraisal: Reflections on Theory, Practice, and Research. *Environment and Planning A*, 36(11), pp.1943–1959.
- Pálsson, G., 1998. The Virtual Aquarium: Commodity Fiction and Cod Fishing. *Ecological Economics*, 24(2–3), pp.275–288.
- Parson, E.A., 1995. Integrated Assessment and Environmental Policy Making: In Pursuit of Usefulness. *Energy Policy*, 23(4–5), pp.463–475.
- Parson, E.A., 1997. Informing Global Environmental Policy-Making: A Plea for New Methods of Assessment and Synthesis. *Environmental Modeling and Assessment*, 2(4), pp.267–279.
- Parson, E.A. & Fisher-Vanden, K., 1997. Integrated Assessment Models of Global Climate Change. *Annual Review of Energy and the Environment*, 22(1), pp.589–628.
- Pastoor, M.A. et al., 2007. Validating Management Simulation Models and Implications for Communicating Results to Stakeholders. *ICES Journal of Marine Science*, 64(4), pp.818–824.
- Patterson, K. & Résimont, M., 2007. Change and Stability in Landings: The Responses of Fisheries to Scientific Advice and TACs. *ICES Journal of Marine Science*, 64(4), pp.714–717.
- Pauly, D. et al., 2002. Towards Sustainability in World Fisheries. *Nature*, 418, pp.689–695.
- Peterson, A.C., 2006. *Simulating Nature: A Philosophical Study of Computer-simulation Uncertainties and Their Role in Climate Science and Policy Advice*, Apeldoorn, the Netherlands: Het Spinhuis.
- Petersen, A.C., 2008. The Practice of Climate Simulation and Its Social and Political Context. *Netherlands Journal of Geosciences*, 87(3), pp.219–229.
- Pickering, A., 1995. *The Mangle of Practice: Time, Agency, and Science*, Chicago: University of Chicago Press.

- Pidgeon, N., 1996. Grounded Theory: Theoretical Background. In J. E. Richardson, ed. *Handbook of Qualitative Research Methods for Psychology and the Social Sciences*. Leicester: British Psychological Society, pp. 75–85.
- Pielke, Jr., R.A., 2006. When Scientists Politicize Science. *Regulation*, (Spring), pp.28–34.
- Pitcher, T.J., Kalikoski, D. & Pramod, G., 2006. Evaluations of Compliance with the FAO (UN) Code of Conduct for Responsible Fisheries. *Fisheries Centre Research Reports*, 14(2), pp.1–75.
- Platt, J., 1981. Evidence and Proof in Documentary Research: 1: Some Specific Problems of Documentary Research. *The Sociological Review*, 29(1), pp.31–52.
- Pohl, C., 2010. Collaboration of Natural and Social Sciences in Research for Sustainable Development. In U. Landfester & G. Pálsson, eds. *The Future of Knowledge – Mapping Interfaces*. Strasbourg: European Science Foundation, pp. 25–34 [online]. Available from: [www.esf.org/fileadmin/Public\\_documents/Publications/Future\\_of\\_Knowledge.pdf](http://www.esf.org/fileadmin/Public_documents/Publications/Future_of_Knowledge.pdf) [Accessed 29 October 2013].
- Polanyi, M., 1962. The Republic of Science: Its Political and Economic Theory. *Minerva*, 1(1), pp.54–73.
- Pollock, N. & Williams, R., 2007. Technology Choice and its Performance: Towards a Sociology of Software Package Procurement. *Information and Organization*, 17(3), pp.131–161.
- Poos, J.J., Machiels, M.A.M. & Pastoors, M.A., 2006. *Investigation of Some Management Scenarios for North Sea Sole and Plaice in 2006 and Beyond*. Ijmuiden, the Netherlands: Centre for Fishery Research, CVO 06.004.
- Pope, J. et al., 2006. *Long-Term Management of North Sea Fisheries: A Report to DEFRA and the North Sea Regional Advisory Council*. Seafish, Edinburgh, UK. Available from: [http://nsrac.org/wp-content/uploads/2009/09/wp04\\_wd20060615\\_Summary\\_LTM\\_Report.pdf](http://nsrac.org/wp-content/uploads/2009/09/wp04_wd20060615_Summary_LTM_Report.pdf) [Accessed 29 October 2013].
- Porter, T., 1995. *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*, Princeton, New Jersey: Princeton University Press.
- Radaelli, C.M., 1999. The Public Policy of the European Union: Whither Politics of Expertise? *Journal of European Public Policy*, 6(5), pp.757–774.

- Radaelli, C.M., 2004. How Context Matters: Regulation Quality in the European Union. In: *54th Annual Conference of the UK Political Studies Association*. Lincoln, UK, 5-8 April 2004.
- Radaelli, C.M., 2005. Diffusion Without Convergence: How Political Context Shapes the Adoption of Regulatory Impact Assessment. *Journal of European Public Policy*, 12(5), pp.924–943.
- Radaelli, C.M., 2007. Whither Better Regulation for the Lisbon agenda? *Journal of European Public Policy*, 14(2), pp.190–207.
- Radaelli, C.M. & De Francesco, F., 2007. Regulatory Impact Assessment, Political Control and the Regulatory State. In *4th General Conference of the European Consortium for Political Research*. Pisa, Italy, 6-8 September 2007.
- Radaelli, C.M. & Meuwese, A.C.M., 2008. Hard Questions, and Equally Hard Solutions? Proceduralization Through Impact Assessment in the European Union. In *CONNEX Workshop on Governing the European Union: Policy Instruments in a Multi-Level Polity*. Norwich, UK, 28-29 May 2008.
- Ragin, C.C., 1992. Introduction: Cases of “What is a Case?” In C. C. Ragin & H. S. Becker, eds. *What is a Case? Exploring the Foundations of Social Inquiry*. Cambridge: Cambridge University Press, pp. 1–18.
- Rayner, S. & Malone, E.L. eds., 1998. *Human Choice and Climate Change: An International Assessment*, Columbus, Ohio: Battelle Press.
- Rayner, S., Lach, D. & Ingram, H., 2005. Weather Forecasts Are for Wimps: Why Water Resource Managers Do Not Use Climate Forecasts. *Climate Change*, 69, pp.197–227.
- Redclift, M., 1998. Dances with Wolves? Interdisciplinary Research on the Global Environment. *Global Environmental Change*, 8(3), pp.177–182.
- Reeves, S.A. & Pastoors, M.A., 2007. Evaluating the Science Behind the Management Advice for North Sea Cod. *ICES Journal of Marine Science*, 64(4), pp.671–678.
- Renda, A., 2006. *Impact Assessment in the EU: The State of the Art and the Art of the State*, Brussels: CEPS Paperbacks.
- Rheinberger, H.-J., 1997. *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Stanford, California: Stanford University Press.

- Rip, A., 2002. Science for the 21st Century. In P. Tindemans, A. Verrijn-Stuart, & R. Visser, eds. *The Future of the Sciences and Humanities*. Amsterdam: Amsterdam University Press, pp. 99–148.
- Rønning, S.B., 2002. *Two Countries Sharing a Renewable Resource – The Allocation of the Renewable Natural Resources in the Norwegian-Russian Fisheries Management Regime of the Barents Sea: A Multidisciplinary Study*. Thesis (MSc). University of Tromsø. Available from: <http://munin.uit.no/bitstream/handle/10037/322/thesis.pdf?sequence=1> [Accessed 29 October 2013].
- Rose, N. & Miller, P., 1992. Political Power Beyond the State: Problematics of Government. *The British Journal of Sociology*, 43(2), pp.173–205.
- Rotmans, J. & Dowlatabadi, H., 1998. Integrated Assessment of Climate Change: Evaluation of Models and other Methods. In S. Rayner & E. L. Malone, eds. *Human Choice and Climate Change: An International Assessment*. Columbus, Ohio: Battelle Press, pp. 85–102.
- Rotmans, J. & Van Asselt, M.B.A., 2000. Towards an Integrated Approach for Sustainable City Planning. *Journal on Multi-Criteria Decision Analysis*, 9(1-3), pp.110–124.
- Rowe, E.W., 2009. *Russia and the North*, Ottawa: University of Ottawa Press.
- Rowell, J. & Mangenot, M. eds., 2011. *A Political Sociology of the European Union: Reassessing Constructivism*, Manchester: Manchester University Press.
- Rozwadowski, H.M., 2002. *The Sea Knows No Boundaries: A Century of Marine Science Under ICES*, Washington D.C.: University of Washington Press.
- Russell, S. & Williams, R., 2002. Social Shaping of Technology: Frameworks, Findings and Implications for Policy. In K.T. Sørensen & R. Williams, eds. *Shaping Technology, Guiding Policy*. Cheltenham: Edward Elgar, pp. 133–154.
- Salter, L., 1988. *Mandated Science: Science and Scientists in the Making of Standards*, Dordrecht, the Netherlands: Kluwer.
- Schaffer, S., 1991. The Eighteenth Brumaire of Bruno Latour. *Studies in History and Philosophy of Science*, 22(1), pp.175–192.
- Schneider, S.H., 1997. Integrated Assessment Modeling of Global Climate Change: Transparent Rational Tool for Policy Making or Opaque Screen Hiding Value-laden Assumptions? *Environmental Modeling and Assessment*, 2(4), pp.229–249.

- Schwach, V. et al., 2007. Policy and Knowledge in Fisheries Management: A Policy Brief. *ICES Journal of Marine Science*, 64(4), pp.798–803.
- Scott, J.C., 1998. *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*, New Haven: Yale University Press.
- Shackley, S., 1997. Trust in Models? The Mediating and Transformative Role of Computer Models in Environmental Discourse. In M. Redclift & G. Woodgate, eds. *International Handbook of Environmental Sociology*. Cheltenham: Edward Elgar.
- Shackley, S., 2001. Epistemic Lifestyles in Climate Change Modeling. In C. Miller & P. N. Edwards, eds. *Changing the Atmosphere: Expert Knowledge and Environmental Governance*. Cambridge, Massachusetts: MIT Press, pp. 107–133.
- Shackley, S. & Darier, E., 1998. Seduction of the Sirens: Global Climate Change. *Climate Change*, 25(5), pp.313–325.
- Shackley, S. & Wynne, B., 1995a. Global Climate Change: The Mutual Construction of an Emergent Science-Policy Domain. *Science and Public Policy*, 22(4), pp.218–230.
- Shackley, S. & Wynne, B., 1995b. Integrating Knowledges for Climate Change: Pyramids, Nets and Uncertainties. *Global Environmental Change*, 5(2), pp.113–126.
- Shackley, S. & Wynne, B., 1996. Representing Uncertainty in Global Climate Change Science and Policy: Boundary-Ordering Devices and Authority. *Science, Technology & Human Values*, 21(3), pp.275–302.
- Shackley, S. et al., 1998. Uncertainty, Complexity and Concepts of Good Science in Climate Change Modelling: Are GCMs the Best Tools? *Climatic Change*, 38(2), pp.159–205.
- Shackley, S. et al., 1999. Adjusting to Policy Expectations in Climate Change Modeling: An Interdisciplinary Study of Flux Adjustments in Coupled Atmosphere-Ocean General Circulation Models. *Climatic Change*, 43(2), pp.413–454.
- Shapin, S. & Schaffer, S., 1985. *Leviathan and the Air-Pump*, Princeton, New Jersey: Princeton University Press.
- Siebenhüner, B. & Barth, V., 2005. The Role of Computer Modelling in Participatory Integrated Assessments. *Environmental Impact Assessment Review*, 25(4), pp.367–389.



- Simon, H.A., 1972. Theories of Bounded Rationality. *Decision and Organization*, 1, pp.161–176.
- Sismondo, S., 1999. Models, Simulations, and their Objects. *Science in Context*, 12(2), pp.247–260.
- Skarstad, G.Å., 2008. Balancing Fish: A Meeting between Food Safety and Nutrition in an Assessment of Benefits and Risks. *Distinktion: Scandinavian Journal of Social Theory* (16), pp.99–114.
- Smith, A., 2013. How the European Commission’s Policies Are Made: Problematization, Instrumentation and Legitimation. *Journal of European Integration*, (October), pp.1–18 [online]. Available from: <http://dx.doi.org/10.1080/07036337.2013.809344> [Accessed 29 October 2013].
- Sørensen, K.H., 1996. *Learning Technology, Constructing Culture. Socio-Technical Change as Social Learning*. Social Learning in Multimedia (SLIM). CEC 4<sup>th</sup> Framework Programme, Project No. PL 951003. Available from: [www.rcss.ed.ac.uk/SLIM/public/phase1/knut.html](http://www.rcss.ed.ac.uk/SLIM/public/phase1/knut.html) [Accessed 30 December 2012]
- Sørensen, K.H., Aune, M. & Hatling, M., 2000. Against Linearity: On the Cultural Appropriation of Science and Technology. In M. Dierkes & C. Von Grote, eds. *Between Understanding and Trust: The Public, Science and Technology*. London: Routledge, pp. 237–257.
- Sparholt, H., Bertelsen, M. & Lassen, H., 2007. A Meta-Analysis of the Status of ICES Fish Stocks during the Past Half Century. *ICES Journal of Marine Science*, 64(4), pp.707–713.
- Spencer, M., 2012. *Reason and Representation in Scientific Simulation*. Thesis (PhD). Goldsmiths, University of London. Available from: [http://eprints.gold.ac.uk/7942/1/CCS\\_thesis\\_Spencer\\_2012.pdf](http://eprints.gold.ac.uk/7942/1/CCS_thesis_Spencer_2012.pdf) [Accessed 29 October 2013].
- Star, S.L., 2010. This is Not a Boundary Object: Reflections on the Origin of a Concept. *Science, Technology & Human Values*, 35(5), pp.601–617.
- Star, S.L. & Griesemer, J., 1989. Institutional Ecology, “Translations” and Boundary Objects: Amateurs and Professionals in Berkeley’s Museum of Vertebrate Zoology, 1907-39. *Social Studies of Science*, 19(3), pp.387–420.
- Stewart, J. & Williams, R., 2005. The Wrong Trousers? Beyond the Design Fallacy: Social Learning and the User. In H. Rohrbacher, ed. *User Involvement in Innovation Processes: Strategies and Limitations from a Socio-technical Perspective*. Munich: Profil Verlag, pp. 39–71.

- Stirling, A., 2008. "Opening Up" and "Closing Down": Power, Participation, and Pluralism in the Social Appraisal of Technology. *Science, Technology & Human Values*, 33(2), pp.262–294.
- Stokke, O.S., 2005. The Struggle Over Illegal, Unreported and Unregulated Fishing in the Barents Sea. *Aftenposten*, (1), pp.1–4.
- Strathern, M., 2002. Externalities in Comparative Guise. *Economy and Society*, 31(2), pp.250–267.
- Strauss, A.L. & Corbin, J., 1990. *Basics of Qualitative Research: Grounded Theory Procedures and Research*, London: Sage.
- Sundberg, M., 2006. Credulous Modellers and Suspicious Experimentalists: Comparison of Model Output and Data in Meteorological Simulation Modelling. *Science Studies*, 19(1), pp.56–68.
- Sundberg, M., 2007. Parameterizations as Boundary Objects on the Climate Arena. *Social Studies of Science*, 37(3), pp.473–488.
- Sundberg, M., 2009. The Everyday World of Simulation Modeling: The Development of Parameterizations in Meteorology. *Science, Technology & Human Values*, 34(2), pp.162–181.
- Sundberg, M., 2012. Creating Convincing Simulations in Astrophysics. *Science, Technology & Human Values*, 37(1), pp.64–87.
- Sundqvist, G. & Letell, M., 2005. Configuring the Urban Traveler: Local Adaptation of European Air Policy. *Acta Sociologica*, 48(3), pp.205–219.
- Sundqvist, G., Letell, M. & Lidskog, R., 2002. Science and Policy in Air Pollution Abatement Strategies. *Environmental Science & Policy*, 5(2), pp.147–156.
- Symes, D., 1997. The European Community's Common Fisheries Policy. *Ocean & Coastal Management*, 35(2-3), pp.137–155.
- Symes, D., 2001. The Future of Europe's Fisheries: Towards a 2020 Vision. *Geography*, 86(4), pp.318–328.
- Symes, D., 2006. Fisheries Governance: A Coming of Age for Fisheries Social Science? *Fisheries Research*, 81(2-3), pp.113–117.
- Symes, D. & Crean, K., 1995. Historic Prejudice and Invisible Boundaries: Dilemmas for the Development of the Common Fisheries Policy. In G. Blake et al., eds. *The Peaceful Management of Transboundary Resources*. London: Graham and Trotman.

- The Fisheries Secretariat, 2007. *Commission Proposes Ban on Discard* [online]. Available from: [www.fishsec.org/2007/03/29/commission-proposes-ban-on-discard](http://www.fishsec.org/2007/03/29/commission-proposes-ban-on-discard) [Accessed 29 October 2013].
- The Norwegian Ministry of Fisheries and Coastal Affairs, 2007. *Agreed Record of Conclusions of Fisheries Consultations between The European Community and Norway for 2008*. The Norwegian Ministry of Fisheries and Coastal Affairs, Oslo, Norway. Available from: [www.regjeringen.no/upload/FKD/Vedlegg/Kvoteavtaler/2008/Norge\\_og\\_EU/Agreed\\_Record\\_of\\_Conclusions,\\_EU\\_og\\_Norge\\_2008.pdf](http://www.regjeringen.no/upload/FKD/Vedlegg/Kvoteavtaler/2008/Norge_og_EU/Agreed_Record_of_Conclusions,_EU_og_Norge_2008.pdf) [Accessed 29 October 2013].
- The Norwegian Ministry of Fisheries and Coastal Affairs, 2011. *Fisheries Collaboration with Russia* [online]. Available from: [www.fisheries.no/resource\\_management/International\\_cooperation/Fisheries\\_collaboration\\_with\\_Russia](http://www.fisheries.no/resource_management/International_cooperation/Fisheries_collaboration_with_Russia) [Accessed 29 October 2013].
- Thomas, G., 2010. Doing Case Study: Abduction Not Induction, Phronesis Not Theory. *Qualitative Inquiry*, 16(7), pp.575–582.
- Toren, C., 1996. Ethnography: Theoretical Background. In *Handbook of Qualitative Research Methods for Psychology and the Social Sciences*. Leicester: British Psychological Society, pp. 102–112.
- Tuinstra, W., 2008. European Air Pollution Assessments: Co-Production of Science and Policy. *International Environmental Agreements: Politics, Law and Economics*, 8(1), pp.35–49.
- Tuinstra, W., Hordijk, L. & Amann, M., 1999. Using Computer Models in International Negotiations: The Case of Acidification in Europe. *Environment*, 41(9), pp.33–42.
- Tuinstra, W., Hordijk, L. & Kroeze, C., 2006. Moving Boundaries in Transboundary Air Pollution Co-Production of Science and Policy under the Convention on Long Range Transboundary Air Pollution. *Global Environmental Change*, 16(4), pp.349–363.
- Van Daalen, C.E., Dresen, L. & Janssen, M.A., 2002. The Roles of Computer Models in the Environmental Policy Life Cycle. *Environmental Science & Policy*, 5(3), pp.221–231.
- Van Egmond, S. & Bal, R., 2011. Boundary Configurations in Science Policy: Modeling Practices in Health Care. *Science, Technology & Human Values*, 36(1), pp.108–130.

- Van Egmond, S. & Zeiss, R., 2010. Modeling for Policy Science-Based Models as Performative Boundary Objects for Dutch Policy Making. *Science Studies*, 23(1), pp.58–78.
- Van Hemert, M., 2013. Taming Indeterminacy: The Co-Production of Biodiversity Restoration, Flood Protection and Biophysical Modelling of Rivers and Coastal Environments. *Science Technology & Society*, 18(1), pp.75–92.
- Van Ittersum, M.J., 2007. Preparing Mare Liberum for the Press: Hugo Grotius' Rewriting of Chapter 12 of *De Iure Praedae* in November-December 1608. *Grotiana*, 26(1), pp.246–280.
- Van der Sluijs, J.P., 2002a. Definition of Integrated Assessment. In R. E. Munn & M. Tolba, eds. *Encyclopedia of Global Environmental Change*. London: Wiley, pp. 249–250.
- Van der Sluijs, J.P., 2002b. A Way Out of the Credibility Crisis of Models Used in Integrated Environmental Assessment. *Futures*, 34(2), pp.133–146.
- Van der Sluijs, J.P. et al., 1998. Anchoring Devices in Science for Policy: The Case of Consensus around Climate Sensitivity. *Social Studies of Science*, 28(2), pp.291–323.
- Van der Sluijs, J.P. et al., 2005. Combining Quantitative and Qualitative Measures of Uncertainty in Model-Based Environmental Assessment: The NUSAP System. *Risk Analysis*, 25(2), pp.481–492.
- Velema, N., 2001. An Overview of the Netherlands Open Sea and Coastal Fisheries: State of the Art. *Fisheries Centre Research Reports*, 9(3), pp.141–161.
- Wallace, D.F., 2009. *This Is Water: Some Thoughts, Delivered on a Significant Occasion, about Living a Compassionate Life*, New York: Little Brown.
- Waterton, C. & Wynne, B., 2004. Knowledge and Political Order in the European Environment Agency. In S. Jasanoff, ed. *States of Knowledge: The Co-Production of Science and Social Order*. London: Routledge, pp. 87–108.
- Weber, M., 1964. *The Theory of Social and Economic Organization*, New York: Free Press.
- Weinberg, A.M., 1972. Science and Trans-Science. *Minerva*, 10(2), pp.209–222.
- Weingart, P., 1999. Scientific Expertise and Political Accountability: Paradoxes of Science in Politics. *Science and Public Policy*, 26(3), pp.151–161.
- White, L., 1962. *Medieval Technology and Social Change*, Oxford: Clarendon Press.

- Wilkinson, D. et al., 2005. *For Better or for Worse? The EU's "Better Regulation" Agenda and the Environment*. Institute for European Environmental Policy, London, UK. Available from: [www.ieep.eu/assets/476/FinalReport\\_pdf10.pdf](http://www.ieep.eu/assets/476/FinalReport_pdf10.pdf) [Accessed 29 October 2013].
- Williams, R., 2006. Compressed Foresight and Narrative Bias: Pitfalls in Assessing High Technology Futures. *Science As Culture*, 15(4), pp.327–348.
- Williams, R. & Edge, D., 1996. The Social Shaping of Technology. *Research Policy*, 25(6), pp.865–899.
- Williams, R., Stewart, J.K. & Slack, R.S., 2005. *Social Learning in Technological Innovation: Experimenting with Information and Communication Technologies*, Cheltenham: Edward Elgar.
- Wilson, D.C., 2003. Examining the Two Cultures Theory of Fisheries Knowledge: The Case of Bluefish Management. *Society & Natural Resources*, 16(6), pp.491–508.
- Wilson, D.C., 2009. *The Paradoxes of Transparency: Science and the Ecosystem Approach to Fisheries Management in Europe*, Amsterdam: Amsterdam University Press.
- Wilson, D.C. & Degnbol, P., 2002. The Effects of Legal Mandates on Fisheries Science Deliberations: The Case of Atlantic Bluefish in the United States. *Fisheries Research*, 58(1), pp.1–14.
- Wilson, D.C. & Hegland, T.J., 2005. *An Analysis of Some Institutional Aspects of Science in Support of the Common Fisheries Policy*. Policy and Knowledge in Fisheries Management (PKFM). CEC 5<sup>th</sup> Framework Programme, Project No. Q5RS–2001-01782. Institute for Fisheries Management. Publication No. 126. Available from: [http://vbn.aau.dk/files/13562579/An\\_Analysis\\_of\\_Some\\_Institutional.pdf](http://vbn.aau.dk/files/13562579/An_Analysis_of_Some_Institutional.pdf) [Accessed 29 October 2013]
- Wilson, D.C., Nielsen, J.R. & Degnbol, P., 2003. *The Fisheries Co-management Experience: Accomplishments, Challenges and Prospects*, Dordrecht, the Netherlands: Springer.
- Winner, L., 1980. Do Artefacts Have Politics? *Daedalus*, 109(1), pp.121–136.
- Winner, L., 1986. *The Whale and the Reactor: A Search for Limits in an Age of High Technology*, Chicago, Illinois: University Of Chicago Press.
- Winsberg, E., 1999. Sanctioning Models: The Epistemology of Simulation. *Science in Context*, 12(2), pp.275–292.

- Winsberg, E., 2010. *Science in the Age of Computer Simulation*, Chicago, Illinois: University of Chicago Press.
- Woolgar, S., 1991. Configuring the User: The Case of Usability Trials. In J. Law, ed. *A Sociology of Monsters: Essays on Power, Technology and Domination*. London: Routledge, pp. 66–75.
- World Wildlife Fund, 2006a. *Position Paper on 2007 TACs and Quotas Proposal for Selected Fish Stocks and Fisheries*. World Wildlife Fund, Brussels, Belgium. Available from: [wwf.panda.org/media\\_centre/publications/?88820/WWF-Position-on-the-EC-2007-Total-Allowable-Catches-TACs-Quotas-Proposal-for-selected-fish-stocks-fisheries](http://wwf.panda.org/media_centre/publications/?88820/WWF-Position-on-the-EC-2007-Total-Allowable-Catches-TACs-Quotas-Proposal-for-selected-fish-stocks-fisheries) [Accessed 29 October 2013].
- World Wildlife Fund, 2006b. *Position Paper on Plaice and Sole Long Term Management Plan*. World Wildlife Fund, Brussels, Belgium. Available from: [http://awsassets.panda.org/downloads/wwf\\_position\\_plaice\\_sole\\_ltm\\_oct\\_2006.pdf](http://awsassets.panda.org/downloads/wwf_position_plaice_sole_ltm_oct_2006.pdf) [Accessed 29 October 2013].
- World Wildlife Fund, 2008. *No Justice for Cod* [online]. Available from: [www.wwf.org.uk/wwf\\_articles.cfm?unewsid=1743](http://www.wwf.org.uk/wwf_articles.cfm?unewsid=1743) [Accessed 29 October 2013].
- Wyatt, S., 2008. Technological Determinism is Dead; Long Live Technological Determinism. In E. J. Hackett et al., eds. *The Handbook of Science and Technology Studies*. Cambridge, Massachusetts: The MIT Press, pp. 165–180.
- Wyatt, S. & Balmer, B., 2007. Home on the Range: What and Where is the Middle in Science and Technology Studies? *Science, Technology & Human Values*, 32(6), pp.619–626.
- Wynne, B., 2003. Seasick on the Third Wave? Subverting the Hegemony of Propositionalism: Response to Collins & Evans (2002). *Social Studies of Science*, 33(3), pp.401–417.
- Wynne, B., 2008. Elephants in the Rooms Where Publics Encounter “Science”? A Response to Darrin Durant, “Accounting for Expertise: Wynne and the Autonomy of the Lay Public.” *Public Understanding of Science*, 17(1), pp.21–33.
- Wynne, B., 2010. Strange Weather, Again Climate Science as Political Art. *Theory, Culture & Society*, 27(2-3), pp.289–305.
- Yearley, S., 1988. *Science, Technology, and Social Change*, London: Unwin Hyman.
- Yearley, S., 1991. *The Green Case: A Sociology of Environmental Issues, Arguments and Politics*, London: Harper Collins.

- Yearley, S., 1992. Green Ambivalence About Science: Authority and the Scientific Legitimation of a Social Movement. *British Journal of Sociology*, 43(4), pp.511–532.
- Yearley, S., 1999. Computer Models and the Public's Understanding of Science: A Case-Study Analysis. *Social Studies of Science*, 29(6), pp.845–866.
- Yearley, S., 2000. Making Systematic Sense of Public Discontents with Expert Knowledge: Two Analytical Approaches and A Case Study. *Public Understanding of Science*, 9(2), pp.105–122.
- Yearley, S., 2005. *Making Sense of Science: Understanding the Social Study of Science*, London: Sage.
- Yearley, S., 2006. Bridging the Science-Policy Divide in Urban Air-Quality Management: Evaluating Ways to Make Models More Robust Through Public Engagement. *Environment and Planning C*, 24(5), p.701.
- Yearley, S., 2009. Sociology and Climate Change after Kyoto: What Roles for Social Science in Understanding Climate Change? *Current Sociology*, 57(3), pp.389–405.
- Yearley, S., 2011. Citizen Engagement with the Politics of Air Quality. In R. Lidskog & G. Sundqvist, eds. *Governing the Air: The Dynamics of Science, Policy, and Citizen Interaction*. Cambridge, Massachusetts: The MIT Press, pp. 253–271.
- Yearley, S., Forrester, J. & Bailey, P., 2001. Participation and Expert Knowledge: A Case Study Analysis of Scientific Models and their Publics. In M. Hisschemöller et al., eds. *Knowledge, Power and Participation in Environmental Policy Analysis*. New Brunswick, New Jersey: Transaction, pp. 349–370.
- Yearley, S. et al., 2003. Participatory Modelling and the Local Governance of the Politics of UK Air Pollution: A Three-City Case Study. *Environmental Values*, 12(2), pp.247–262.
- Yin, R.K., 2009[1984]. *Case Study Research: Design and Methods* 4th ed., London: Sage.
- Young, O.R., 2002. *The Institutional Dimensions of Environmental Change: Fit, Interplay, and Scale*, Cambridge, Massachusetts: MIT Press.